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Thirty Years Since Parity Nonconservation

T. D. Lee

Thirty Years Since Parity Nonconservation

A Symposium for T.D. Lee

Edited by Robert Novick

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SYMPOSIUM

TO CELEBRATE THE SIXTIETH BIRTHDAY

OF T. D. LEE

AND TO COMMEMORATE

THE THIRTIETH ANNIVERSARY

OF PARITY NONCONSERVATION

INTRODUCTION

To celebrate the sixtieth birthday of T.D. Lee and to commemorate the thirtieth anniversary of parity nonconservation, the Physics Department at Columbia University held a symposium on November 22, 1986. (This date coincided with the 21st day of the 10th moon of the Year of the Tiger, which is T.D.'s exact Chinese birthday.) Nearly 300 physicists participated. We are grateful to them for taking part in this joyful event. The warmth and respect that the international community of physicists holds for T.D. was amply demonstrated on this occasion, and the entire Physics Department takes pride in it.

Nowadays, with broken symmetry almost universally accepted as a way of life, it is perhaps difficult for a modern student of physics to realize the basic taboo of the past period. Thirty years ago, it was unthinkable (certainly to me) that anyone should question the validity of symmetries under space inversion, charge conjugation and time reversal. It would have been almost sacriligious to do experiments to test such "unholy" thoughts. We are particularly delighted that the original scientists who did the experiments were able to give their personal accounts at this symposium. To complete the record, T.D. has written a reminiscence giving his recollection of the theoretical side of events.

Since 1979, T.D. has spent much time organizing and running the CUSPEA (China-U.S. Physics Examination and Application) program. Ninety physics departments in American universities have joined this project. Altogether, nearly 800 physics graduate students were selected from China and placed with these schools. They are excellent students and will undoubtedly play a significant role in physics in general and in the scientific development of China. On November 23rd, the day after the departmental celebration, the symposium was continued by the CUSPEA students; representative results of their research were presented. This proceedings includes the talks given during both days of the symposium. The combination of the past and the future makes it a unique tribute to T.D. as well as a living testimonial to the vigor of our discipline.

Robert Novick

A Moment between the Sessions

L. M. Lederman, J. Steinberger, T. D. Lee, W. K. H. Panofsky

SYMPOSIUM ATTENDEES

November 22, 1986

Stephen L. Adler	Institute for Advanced Study
James L. Anderson	Stevens Inst. of Technology
Elena Aprile	Columbia University
John Bahcall*	Institute for Advanced Study
Charles Baltay	Columbia University
Karen Barad	Columbia University
M. A. B. Bég	Rockefeller University
Jeremy Bernstein	Stevens Inst. of Technology
Joseph Birman	City College of the CUNY
James D. Bjorken	Fermi Nat'l Accelerator Laboratory
Allan S. Blaer	Columbia University
Sidney Bludman	University of Pennsylvania
Martin Blume	Brookhaven National Laboratory
Frank Brown	Columbia University
Laurie Brown	Northwestern University
Nicola Cabibbo	Ist. Nazionale Fisica Nucleare
Leslie Camilleri	Columbia University
Carl Carlson	College of William and Mary
Kenneth Case	Rockefeller University
S. Chandrasekhar	University of Chicago
Chao-Hsi Chang	Fermi Nat'l Accelerator Laboratory and Institute of Theoretical Physics, Beijing
Lay Nam Chang	Virginia Polytechnic Institute and State University
Ngee-Pong Chang	City College of the CUNY
Shau-Jin Chang	University of Illinois
Georges Charpak	C E R N
Ling-Lie Chau	University of California, Davis
Min Chen	Massachusetts Inst. of Technology
Hung Cheng	Massachusetts Inst. of Technology
Ta-Pei Cheng	University of Missouri
K. C. Chou	Chinese Academy of Sciences
Max Chrétien	Brandeis University
Norman H. Christ	Columbia University
E. D. G. Cohen	Rockefeller University
Sidney Coleman	Harvard University
Rodney Cool	Rockefeller University
Robert Crease	Columbia University
James W. Cronin	University of Chicago

*Those who registered by mail in advance, but did not actually attend, are designated by an asterisk.

Richard J. Drachman	Nat'l Aeronautics & Space Adm.
Sidney D. Drell	Stanford Linear Accelerator Ctr
Anthony Duncan	University of Pittsburgh
Freeman Dyson	Institute for Advanced Study
Gösta Ekspong	University of Stockholm
Glennys R. Farrar	Rutgers University
Gerald Feinberg	Columbia University
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Douglas B. Fitchen	Cornell University
Earle C. Fowler	U.S. Department of Energy
Allan Franklin	University of Colorado
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Paolo Franzini	Columbia University
Hans Frauenfelder	University of Illinois
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Xinbai Huang	State Education Commission of China
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Roman Jackiw	Massachusetts Inst. of Technology
Maurice Jacob	C E R N
C. K. Jen	Johns Hopkins University
Kai Jiang	Chinese Consulate, New York
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Martin Klein	Yale University
Ellis Kolchin	Columbia University
Earl L. Koller	Stevens Inst. of Technology
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James Lee	California Inst. of Technology
Stephen Lee	University of Nantes
Wonyong Lee	Columbia University
George Y.-C. Leung	Southeastern Massachusetts Univ.

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Ling-Fong Li	Carnegie-Mellon University
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Keh-Fei Liu	University of Kentucky
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Daniel Lu	Yale University
Elihu Lubkin	University of Wisconsin, Milwaukee
J. M. Luttinger	Columbia University
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Samuel MacDowell	Yale University
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Morgan May	Brookhaven National Laboratory and Columbia University
A. C. Melissinos	University of Rochester
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Shoji Nagamiya	Columbia University
Yoichiro Nambu	University of Chicago
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Uriel Nauenberg	University of Colorado
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Robert Novick	Columbia University
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Michael Ram	SUNY at Buffalo
Malvin Ruderman	Columbia University
Allan M. Sachs	Columbia University
Robert Sachs	University of Chicago
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Larry Spruch	New York University
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George Trilling	Univ. of California, Berkeley
Tran Truong	Ecole Polytechnique
Yung Su Tsai	Stanford Linear Accelerator Ctr
San Fu Tuan	University of Hawaii
Wu-Ki Tung	Illinois Inst. of Technology
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Chien-Shiung Wu	Columbia University
Sau Lan Yu Wu	University of Wisconsin
Tai Tsun Wu	Harvard University
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J. Zinn-Justin	Centre d'Etudes Nucléaires, Saclay
Bruno Zumino	University of California, Berkeley
Daniel Zwanziger	New York University

PART 1

- The Sessions -

November 22, 1986

INTRODUCTORY REMARKS

Jack Steinberger

CERN

I will usurp this honor which has been given me to chair one of the sessions here to say a few words in appreciation of Professor Lee. I am not qualified, and I am not going to discuss his many very fundamental contributions to physics, but I do not think I can let the occasion go past without acknowledging my great indebtedness to T.D. and the privilege which it has been to have been his colleague for so long. We were students together at the University of Chicago. Then we were professors together in this splendid institution and in more recent years I have seen him a lot at CERN. I do not know anyone who has been more helpful to me in my own work, who has been more encouraging. More than once he has suggested very splendid experiments, some of which I have had the sense not to do, and some of which if I could have done them, then maybe I would be sharing the Nobel Prize. His passionate way of going at his physics has been certainly very guiding for me also. So I would just like to express my personal appreciation to Professor Lee.

Now I will introduce the first speaker at the colloquium. It is Professor Melvin Schwartz, with whom I have been privileged to have a very long association. We were collaborators here since the middle fifties and worked together at Nevis for a period of about ten years. Since then we have remained friends, which is a testimony to Professor Schwartz's patience and generosity. He has had a career in physics which is marked by great insight and innovativeness, thinking of new things. In the early days, he engaged himself on the study of the new strange particles, which needed to be understood. Then he noticed that one could do experiments with high energy neutrinos. These experiments resulted, as you know, in the understanding that the mu neutrino is different from the electron neutrino, a very important result. He has also done very beautiful experiments on K^{o} decay, bearing on CP violation, and one extremely ingenious one to look for pi muonic atoms. More recently, he has put his inventive genius to work in industry, by creating his own informatics company. Professor Schwartz is one of those outstanding intellects whose association makes it worthwhile to be a physicist. I look forward very much to hearing from Mel.

J. Steinberger, M. Schwartz, T. D. Lee, L. M. Lederman

J. Steinberger, M. Schwartz, E. Lederman, M. Jacob

EARLY PARITY EXPERIMENTS IN HYPERON DECAY AND OTHER RECOLLECTIONS OF COLUMBIA IN THE FIFTIES

Melvin Schwartz

Digital Pathways, Inc.

My aim today is to reconstruct as best I can my recollections of a truly remarkable ten-year period at Columbia--the years from 1954 to 1964. To those of us who particpated in that era, it must remain the high point in our lives. It was a period of unparalleled excitement and achievement, at the center of which was, of course, our teacher and the source of our inspiration--T. D. Lee.

The early fifties in particle physics was the period during which strange particles--kaons and hyperons-- were produced abundantly enough to study in some detail. The Cosmotron at Brookhaven had just come on-line and a number of groups were engaged in trying to produce and measure the properties of these strange particles. In particular, there was one outstanding problem which occupied the thinking of most particle physicists, theorists and experimentalists alike--the τ-θ puzzle.

The symbols τ and θ are no longer used in the same context as they were in those days. Simply put, the $\tau^{\pm}$ was the kaon which decayed into $\pi^{\pm}\,\pi^{+}\,\pi^{-}$ and the $\theta^{\pm}$ was the kaon which decayed into $\pi^{\pm}\,\pi^{o}$. The burning question was, of course, are they different manifestations of the same particle?

We summarize the state of knowledge in 1954-55 about the $\theta^{\pm}$ and $\tau^{\pm}$:

1. Both particles had more or less the same mass, within the limits of resolution of the experiments.

2. Dalitz had shown the way of determining the spin of the τ by means of a density plot involving the relative momentum in the decay. It was quite clear that the τ was a particle of spin 0. It must then be 0^{-}. If the θ had spin 0 then it must be 0^{+}.

3. The ratio of τ's to θ's at various production angles was determined to be a constant. Thus the production mechanism for θ and τ must be essentially identical.

4. Most astonishing of all, the τ and θ lifetimes were identical even though they had quite different decay modes with substantially differing phase space.

In summary, were it not for the fact that one appeared to be a scalar and the other a pseudoscalar, any rational person might have concluded that the τ and θ were really the same.

Now the notion that parity was not conserved in these decays was not accepted easily. Indeed, Lee was a coauthor of two other papers attempting alternative explanations of the $\tau - \theta$ puzzle. One, a paper by Lee and Orear, set up a γ-ray cascade from a longer-lived χ meson to shorter-lived kaons. The other, proposed by Lee and Yang and called parity doubling, required that there be mirror even-odd parity doublets whenever strangeness was odd. Needless to say, no Lee-Orear γ-ray was ever found. The parity doubling scheme foundered because it could never explain the lifetime equality of the θ and τ .

My own first introduction to the issue of parity violation occurred in the spring of 1956. Nick Samios, Jack Leitner and I were graduate students working with Jack Steinberger on the first detailed study of strange particle production and decay in a bubble chamber. Jack had just returned from the Rochester Conference at which he had presented our results on the processes

$$\pi^- + p \rightarrow \Lambda^0 + K^0 \; ; \qquad \Lambda^0 \rightarrow \pi^- + p$$

and

$$\pi^- + p \rightarrow \Sigma^- + K^+ \; ; \qquad \Sigma^- \rightarrow \pi^- + n \; .$$

One of the questions being addressed at that time related to the spin of the Λ^0 or Σ^- . By studying the angular correlations between the production and decay planes one might determine if the Λ^0 (or Σ^-) had spin 1/2 or 3/2.

In particular, consider the normal to the production plane $\hat{n}_P$ in the direction $\vec{p}_\pi \times \vec{p}_\Lambda$. Consider the normal to the decay plane in the Λ^0 center of mass $\hat{n}_D$ along $\vec{p}_\Lambda \times \vec{p}_P$. Define the angle ϕ as

$$\cos \phi \equiv \hat{n}_P \cdot \hat{n}_D \; .$$

This angle was called the dihedral angle between the production and decay planes. If the Λ^0 had spin 1/2, then ϕ should be distributed uniformly. If, on the other hand, the Λ^0 had spin 3/2 and was produced with some polarization, then the distribution should be of the form

$$D(\phi) \propto 1 + \alpha \cos^2 \phi \; .$$

We had plotted our data for ϕ between 0 and π , although clearly $D(\phi) = D(\pi - \phi)$ and we should have plotted between 0 and $\pi/2$.

In any case I remember quite clearly that, immediately after the Rochester Conference (which took place April 3-7), Jack Steinberger came back to Nevis and told us that while he was discussing the dihedral angle distribution with T. D. Lee after the meeting, T. D. had an interesting idea. He had suggested to Jack that we plot the data from $\phi = 0$ to $\phi = 2\pi$. In particular the data from $\phi = 0$ to $\phi = \pi$ would correspond to the proton from Λ^o decay going in the general direction of $\hat{n}_p$ and the data from $\phi = \pi$ to $\phi = 2\pi$ would correspond to the proton going generally opposite to $\hat{n}_p$. If there were an asymmetry here, then it would be a prima facie example of parity violation in Λ^o decay.

Needless to say we replotted the data, but there was just not enough to come to any conclusion.

Like many great ideas the notion that parity was not conserved in the weak interactions was perking around for a year or so. Neither Lee nor Yang was the first to raise the issue. The key, however, was coming up with a next step. How could the issue be resolved? What experiments could lead to a testing of the hypothesis? Indeed, to my knowledge, the very first suggestion along this line came in that conversation between Jack and T.D. when he proposed that we examine our data for an asymmetry.

At this point in time the next major step, which Lee and Yang took, was a detailed study of the literature to see if the vast amount of work on β decay could have shown up a parity violation. Needless to say, they discovered that there was in fact nothing in any of the β decay spectra or rate measurements that could be indicative of this type of violation. Indeed one must always look for a pseudoscalar type of parameter in order to see such an effect, and none had ever been studied.

Lee and Yang, then convinced that there was no evidence either for or against parity violation in weak interactions (other than the τ-θ puzzle), developed a series of experimental proposals which could illuminate the issue. The rest is history. In 1956 Chien-Shiung Wu and her collaborators discovered that parity was not conserved in β decay. Subsequent experiments by Lederman and Garwin and by Telegdi extended this to the $\pi - \mu - e$ chain. Indeed, it was not until almost eight months later that the Alvarez group and our own large collaboration succeeded in finding the asymmetry in Λ^o decay that extended parity violation to the decay of strange particles. By that time, parity violation was already "old hat".

As you can see, being a graduate student at Columbia in the mid fifties was a remarkable experience.

The next phase of my recollection of the important role that T.D. played in my life begins in the fall of 1959. I was at that time a young assistant professor

doing most of my research at Nevis, but coming down to the campus to teach classes and participate in other activities. Whenever I was down in the afternoon, I made it a practice to come to the afternoon coffee hour on the 8th floor. Invariably, T.D. was at the blackboard leading a discussion in which everyone was excitedly participating. I remember very clearly one such afternoon at which the subject of discussion was the study of weak interactions at high energy. Various ideas were being evaluated, but none seemed useful. That evening, as a direct result of T.D.'s stimulation, I came up with the idea of using neutrinos as a probe, which eventually led to the completion of the first high energy neutrino experiment in 1962.

What I want to emphasize is not only T.D.'s role as the inspirational piece of sand in the oyster, but also his continuing crucial role in focussing one's attention on the major potential result of the experiment, namely the identity of the muon neutrino. Prior to this point in time, Feinberg had pointed out, in a rather important paper, that the absence of $\mu \rightarrow e + \gamma$ well below the level of 10^{-5} of the muon decay was not explicable if there was an intermediate boson, unless there were two types of neutrinos. (This was, in fact, used for a while as an argument against the boson.) In the summer of 1960, at the Rochester Conference, Lee made the <u>essential</u> observation that <u>any</u> mechanism that would be invented to give the weak interaction some inherent size <u>must</u> of necessity lead to $\mu \rightarrow e + \gamma$, unless there were two neutrinos. Furthermore, the inherent size must correspond to <u>less</u> than 300 GeV or else unitarity would be violated in the weak interactions. This would lead to the same 10^{-5} branching ratio as the Feinberg paper. In short, Lee proved in 1960 that there <u>must</u> be two neutrinos and our experimental result was in fact not a surprise at all.

To summarize, T.D. was the great guiding spirt in the world of particle physics in that great decade at Columbia. He gave me a personal sense of direction which I never found anywhere else since then. I must then use this opportunity to extend my thanks for having made that period possible.

G. C. Wick, R. Cool, M. Morita, T. D. Lee, Y. S. Tsai

T. D. Lee and R. Marshak

W. K. H. Panofsky, T.-Z. Ny, T.-Y. Wu

N. P. Chang, K. Y. Ng, K. Q. Shen, D. Mo, L. Mo
X. D. Xie, T.-Z. Ny, T. D. Lee, T.-Y. Wu, K. C. Chou, H. Wang, J. Lee

INTRODUCTORY REMARKS

Ta-You Wu

Academia Sinica, Taipei

It has given me a great honor to be asked to chair this session. Actually, I would have hesitated to accept this invitation had it not been for the fact that Dr. Lee is a personal friend and Professor Wu is also a personal friend for almost forty years. Now to say any words about Professor Lee's work is just as unnecessary as to point out to anyone else that the sun is so bright and it has given out so much light. It is completely unnecessary and therefore I shall say just a few words of a personal nature. Now, I knew Dr. Lee forty-two years ago when he was eighteen. I had the good fortune of really meeting him and also the good fortune of having an opportunity to give him sort of a government fellowship to come to the United States when he had only finished his sophomore year. He hadn't graduated from the university yet. When I came here and he came here in 1946 after the war, I couldn't put him into any university graduate school because he wasn't graduated. I wrote a few letters and one letter I wrote was to Professor Goudsmit. At that time he was with Northwestern University and of course I wrote some words about Lee as a young boy. I said, Lee, he is so bright and I have discovered that he has strange insight in attacking problems, and so on. Anyway I tried to get some help to get Lee into a graduate school. But somehow this letter to Goudsmit leaked out; I did not know how. A few years later I read an article in the New Yorker, by Jeremy Bernstein, where he quoted a passage in my letter. He said that that was the understatement of the century. Well, I am very glad anyway today that I was wrong, that I made the understatement of the century.

Professor Wu, of course, I also knew at Columbia in 1947 when I came here. We were both here, she working in the basement, I on the tenth floor trying to renovate an old--I think the first--molecular beam apparatus of Professor Rabi. Anyway I did not do much with it; I fooled around a little bit. Well, today gives me a great deal of personal happiness and pleasure to be able to be here on this occasion to see Dr. Lee and Professor Wu; both are being celebrated. Professor Wu.

T. D. Lee, C. S. Wu, L. Yuan, T.-Y. Wu

THE DISCOVERY OF NONCONSERVATION OF PARITY IN BETA DECAY

Chien-Shiung Wu
Columbia University

Prologue

Today's Symposium is indeed an unusual and special cause for celebration in that it commemorates two events. This double bill event marks two occasions that are intimately related, namely the birthday of Professor T.D. Lee and the anniversary of the discovery of parity non-conservation.

While our well known and forever youthful looking University Professor T.D. Lee has advanced to the venerable age of three score, everyone will agree that he has not slowed down the smallest amount in his strenuous and active schedule of research and teaching. Neither the brilliance nor clarity of his eloquent oratory has diminished a trace. On the contrary, he has diverted his efforts and time to work for a very important cause, the exchange program known as CUSPEA which enables qualified Chinese students in physics to come to this country for advanced study and training. Thus we are very happy to have such an appropriate occasion as today to express our fondness for his enlightening friendship and to wish both Professor Lee and his charming wife, Mrs. Jeannette Lee the best of health and many happy returns of this day.

As for the commemoration of the thirtieth anniversary of the discovery of parity non-conservation, some people may not think that thirty years is a sufficiently ancient period in an historical sense especially since the discovery of parity's non-conservation happened so swiftly and was so surprising. Many of our friends and guests present in this hall today have actually lived through that exciting and enthusiastic period and have contributed significantly to further developments of parity non-conservation and its broad and useful applications. Let's hope that today's reunion with our comrades may rekindle our aspirations for tomorrow.

Symmetry and Bilateral Symmetry

Symmetry: The general concept of symmetry has a broad meaning and has occupied a very important position in the history of human civilization.

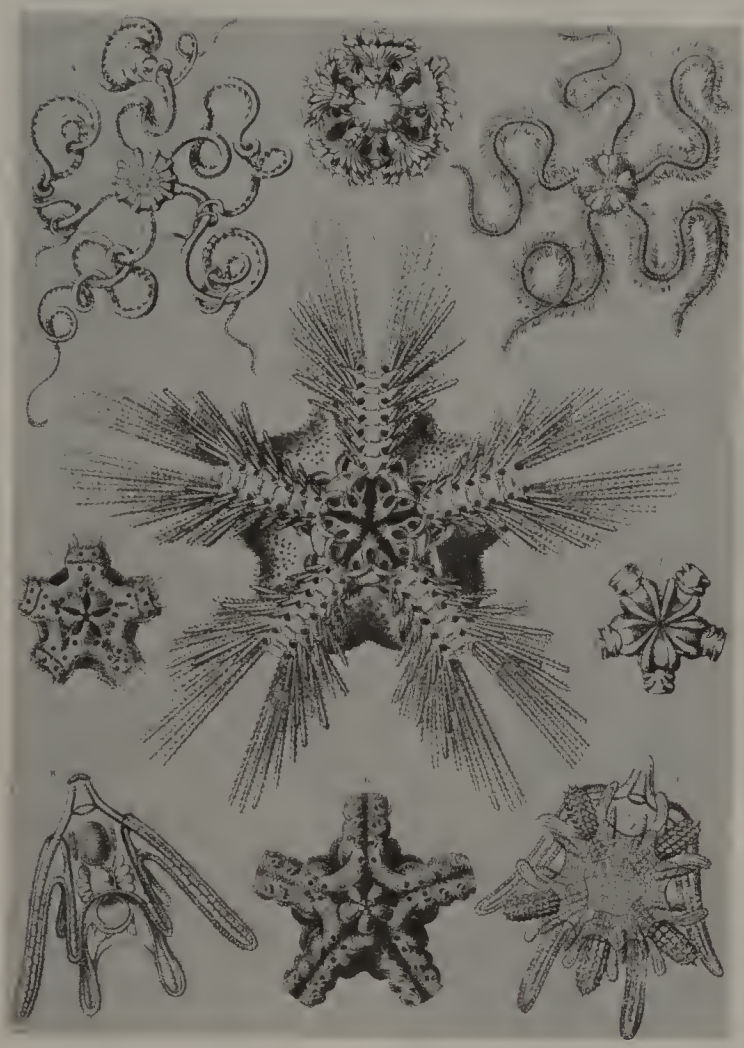
Bilateral Symmetry: The left-right symmetry is a bilateral symmetry and also a very prominent one. When the well known mathematician, Professor Hermann Weyl, retired from Princeton University only a few years before the question of parity became the focal point in the theory of particle physics, he gave a series of four beautiful lectures to expound on the subject of symmetry and presented their forms, meanings and corresponding variance elements. ["Symmetry" by Hermann Weyl, Princeton University, Princeton, New Jersey (1952)]

A Few Illustrations of Various Forms of Symmetry

A few selected illustrations shown in Fig. 1 - Fig. 8 may give you some idea that symmetry is indeed present everywhere in nature as well as in works of art and architecture. From such popular consensus and conviction, it is not difficult to imagine how one's ancestors came to appreciate the idea of symmetry, particularly the simple bilateral symmetry.

Nature Phenomena

Fig. 1 Snowflakes or little marvels of frozen water are the best known specimens of hexagonal symmetry. They were the delight of old and young.



Nature Phenomena

Fig. 2 This figure shows cyclic symmetry of 5 of echinoderma from Ernest Haeckel's 'Kunstformen der Natur.' Their larvae are organized according to the principle of bilateral symmetry.

Works of Art

Fig. 3 This figure of a Greek sculpture of a noble praying boy can be used to illustrate bilateral symmetry.

Works of Art

Fig. 4 A bronze statue from Northern Wei dynasty (385-534) in China also represents bilateral symmetry.

Works of Art

Fig. 5 The Sumerians seem to have been particularly fond of strict bilateral symmetry. Alas! in this picture the two eagle-headed men are nearly but not quite symmetrical. (look at their arms)

Works of Art

Fig. 6 Bronze wall "Gui," Shang dynasty.

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Architecture
Fig. 7 The rear view of the Romanesque cathedral in Mainz. Here shows repetition in the round arcs of the friezes, octagonal central symmetry in the small rosetta and the three towers, while bilateral symmetry rules the structure as a whole as well as almost every detail.

Architecture
Fig. 8 The Palace of the Doges in Venice may stand for translatory symmetry in architecture.

Symmetry in Sciences:

Since symmetry occupies such an important position in the history of human civilization, philosophers and scientists have naturally attempted to make use of this idea, but the attempts have met with little success. For instance, in 1595 Kepler tried to use the symmetry of geometrical structure to explain the ratio of the diameters of planetary orbits (see Fig. 9). The six spheres correspond to the six planets; Saturn, Jupiter, Mars, Earth, Venus, and Mercury, separated in this order by cube, tetrahedron, dodecahedron, octahedron, and icosahedron. This unsuccessful way of seeking harmony in static forms such as in regular solids by Kepler was done long before his famous discovery of the three dynamic laws which now bear his name.

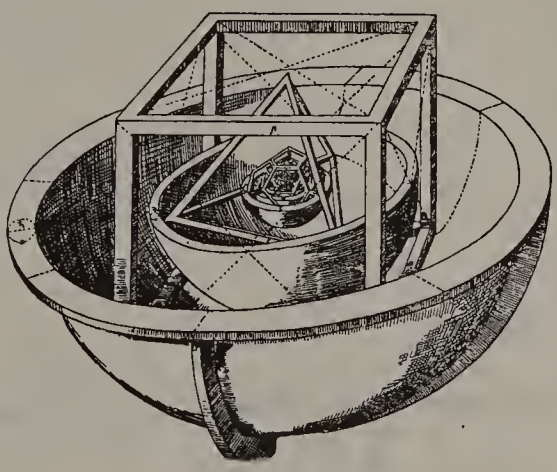

Fig. 9 Kepler made an attempt to deduce the distances in the planetary system to regular bodies which are alternatingly inscribed and circumscribed to spheres. This figure was published in 1595 in his 'Mysterium Cosmographicum' by which he believed he had penetrated deeply into the secrets of the Creator.

By the 19th century, the idea of symmetry had become the central theme in a number of modern scientific disciplines; the obvious ones being crystallography (see Fig. 10), then molecular, atomic, nuclear, particle physics, chemistry and so forth. The importance of symmetry principles eventually dictated the types of fundamental interactions among various elementary particles.

Conservation Laws and Symmetry:

The first and perhaps the most important concept of symmetry is that space and time are isotropic and homogeneous. All points and all directions in space are equivalent so that there is no real distinction of

absolute location in time and space. These are known as symmetry principles or equivalently invariance principles. The basic laws of conservation of momentum and energy are the direct consequences of the invariance of physical laws under space and time displacements. In other words, "symmetry and conservation laws are really one and the same thing." (see Fig. 11)

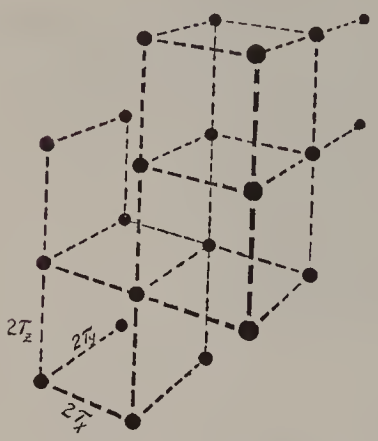

Fig. 10 Crystal structure is closely related to symmetry and invariance elements. A crystal lattice has a large number of invariance elements such as displacing to the right by one unit, or upwards by one unit, etc.

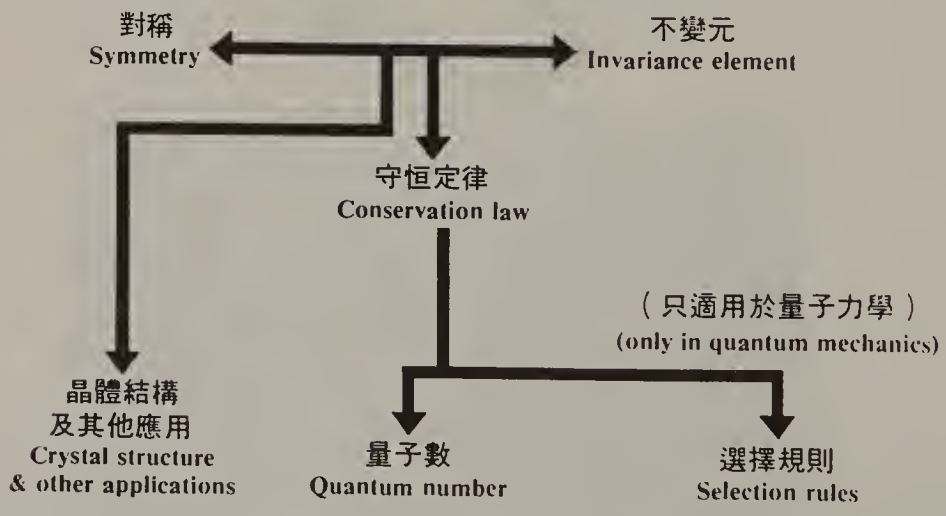

Fig. 11 Symmetry and conservation laws are really one and the same thing. After the introduction of quantum mechanics in discussing the structure of atoms or molecules, we must refer to their quantum numbers, the idea of which has its roots in symmetry principles.

Left and right being a discrete symmetry, therefore the laws of right-left symmetry did not play an important part in classical physics. It came to its eminence with the introduction of quantum mechanics. In fact, the conservation of parity is the direct consequence of the law of left-right symmetry.

The Law of Parity Invariance states that for any atomic or nuclear system, no new physical law should result from the construction of a new system differing from the original by being a mirror image. That is, there is no absolute distinction between a real object (or event) and its mirror image. In other words, two worlds, one based upon a right-handed system (say, real object) and one based upon a left-handed system (say, mirror image) obey the same laws of physics. This law had been built into all physical theories from the 1920's to 1957 and has severely restricted the predicted behavior of elementary physics.

The "Tau" and "Theta" Puzzle:

Up until about 1956 all theoretical physicists accepted the validity of parity conservation. No experimentalists ever thought of devising tests to challenge its validity.

Then, the big puzzle in K-meson decay came onto the scene to stun the experts. K-mesons are unstable particles which were discovered in 1952-1953. Some K-mesons decay into two π-mesons, others into three π-mesons. The k-mesons yielding two π-mesons are called "Theta"; those yielding three π-mesons are called "Tau". "Tau" and "Theta" are identical twins of the same mass and the same lifetime.

$$\theta \rightarrow 2\pi$$

$$\tau \rightarrow 3\pi$$

The decays into two or three π-mesons are permitted by theory and therefore, it is nothing startling. But the two decay modes cannot be reconciled with the law of conservation of parity. "Tau" decays to an odd number of π-mesons of odd parity; "Theta" to an even number of π-mesons of odd parity. If one of them observes the conservation of parity, the other must violate it. So deeply rooted was the conception of parity that the physicists involved in the k-meson problem were greatly puzzled by this riddle.

The Question of Parity Conservation in Weak Interactions:

By April of 1956, various participants in the sixth Rochester Conference began to express their doubts about the Universal Validity of Parity. Lee and Yang immediately plunged into a systematic investigation of the status of experimental knowledge concerning the parity conservation and were surprised that although parity was conserved in strong interactions, no experiments had ever been designed specifically to test such an invariance in weak interactions.

The weak interactions include β-decay, π-meson decay and μ-meson decay and strange particle interaction.

So, the anxiety that began with a simple isolated puzzle in "Tau" and "Theta" cases is now applied to a broad and pressing question: "Is Parity Conserved in All Weak Interactions? "

Conservation of Parity Operation in Radioactive Decays:

To understand the meaning of the experiment on polarized nuclei, one must first examine the meaning of conservation of parity in radioactive decays. The Law of Parity dictates that the physics phenomena of atomic or nuclear systems in the original and its mirror twin should be indistinguishable. Fig. 12 shows the mirror reflection of a spinning ball. If the ball ejected particles equally in both directions along its axis, the image and the real object could not be distinguished because the top right one looks just like the real ball turned upside down.

However, if there is a preferred direction for the ejection of particles, then the mirror reflection can be distinguished. The mirror image at the bottom cannot be mistaken for the real thing, as they have reversed handedness.

β Particle Distribution About Nuclear Axis:

Mathematically, it states that a pseudoscalar term $\langle \sigma \cdot p \rangle$, changes sign under space inversion where p is the electron momentum and σ the spin of the nucleus. If the distribution of emitted electrons from polarized nuclei is asymmetrical (see Fig. 13), the pseudoscalar term $\langle \sigma \cdot p \rangle$ of the radioactive decay is not identically equal to zero.

The pseudoscalar term $\langle \sigma \cdot p \rangle \neq 0$ will change sign under space inversion. Therefore, the parity is not conserved.

My Deep Feeling For The Beta Decay:

During the years 1945-1952, I was completely submerged in the experimental studies of beta decay. From 1952 on, my interest gradually turned away from beta decay. To me, beta decay was still like a dear old friend. There would always be a place in my heart reserved especially for it.

This feeling was rekindled when, one day in the early spring of 1956, my colleague T.D. Lee came up to my office on the thirteenth floor of Pupin Pysics Lab. He asked me a series of questions concerning the status of the experimental knowledge of beta decay.

Unfortunately, I could not supply him with any information on the pseudoscalar quantity $\langle \sigma \cdot p \rangle$ from experimental results of β decay. All the previous β-decay experiments investigated were essentially "only" scalar quantities, for example, the shape of the β-spectrum and the intensities of

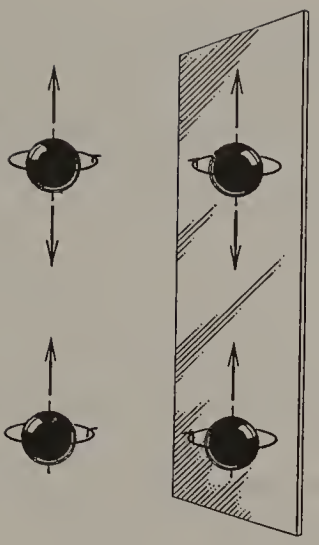

Fig. 12 The mirror reflection of a spinning ball. The image and the real object could not be distinguished because the top right one looks just like the real ball turned upside down. Reflection can be detected if there is a preferred direction.

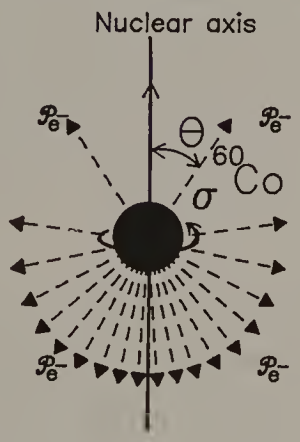

Fig. 13 σ, the spin of the nucleus; p_e the electron momentum. If parity conservation is valid; the expectation value of $(\sigma \cdot p_e) \equiv 0$.

$$\langle(\sigma \cdot p_e)\rangle \equiv \int dr \psi^*(r)[\sigma(r) \cdot P(\gamma)]\psi(r)$$

If parity invariance is valid; then P-operation gives $P\psi(r) = \phi(-r) = \pm\psi(r)$

then
$$P\langle(\sigma \cdot p_e)\rangle = \int d\psi^*(r)[\sigma(-r) \cdot p_e(-r)]\psi(r)$$
$$= -\int dr \psi^*(r)[\sigma(r) \cdot p_e(r)]\psi(r)$$
$$= -\langle(\sigma \cdot p_e)\rangle$$

If parity invariance is valid, $\langle(\sigma \cdot p_e)\rangle \equiv 0$, the expectation value $\langle\rangle$ of pseudoscalar quantity must be identically zero.

half lives, etc. Before T.D. left my office, I asked him whether anyone had any ideas about doing this test. He said that some people had suggested using polarized nuclei produced during nuclear reactions or using a polarized slow neutron beam from a reactor. Somehow, I had great misgivings about using either of these two approaches. I suggested that the best bet would be to use a ^{60}Co β-source polarized by the adiabatic demagnetization method, by which one could attain a polarization as high as 65%. Dr. Lee was very much interested in the possibility of such a strongly polarized ^{60}Co β-source and asked me to lend him a reference book on the method.

Polarized ^{60}Co Experiment - Adiabatic Demagnetization:

In this Demagnetization Method, the principle of polarization is based upon the fact that in certain paramagnetic salts, there are large magnetic fields ($\sim 10^5$-10^6 gauss) at the nuclei of the paramagnetic ions due to unpaired electrons and, at temperatures of the order of 0.01^0k, the nuclear magnetic moments become oriented with respect to these electron magnetic fields. Since the electron magnetism is easily saturated at low temperature, a field of a few hundred gauss suffices. Nuclear orientation will automatically follow. Because of my familiarity with the capability and limitations of this technique, it was only natural that the first thought which came to my mind was to use the polarized ^{60}Co source.

My Decision to Go Ahead:

As an experimentalist, I was also challenged by two techniques which had never been tried before and that were difficult. One was to put an electron detector inside a cryostat at a liquid helium temperature and to make it function as a β-spectrometer; the other was to fabricate a β-source located in a very thin surface layer and have it stay polarized for a time period long enough to obtain sufficient statistics.

In order to do the demagnetization method, one needs very complicated ultra-low temperature equipment. There were only two or three low temperature labs in the United States which were equipped to do nuclear orientation experiments. Dr. Ernest Ambler, a pioneer in the nuclear orientation field had moved from Oxford University to the Low Temperature Lab at the National Bureau of Standards years earlier. I decided to contact him by phone to determine whether he would be interested in a collaboration, although we had never met prior to this.

It was on June 4, 1956 that I called and put the proposition directly to him. He accepted immediately and with enthusiasm. In the middle of September my preparations were ready. I finally went to Washington, D.C. for my first meeting with Dr. Ambler. He was exactly as I had imagined him to be from our numerous telephone con-

versations; soft-spoken, capable and efficient. By the time of my third trip to Washington, D.C. I had grown two ^{60}Co specimens.

One was made by taking a good single crystal of CMN (cerium magnesium nitrate) and growing on the upper surface only, an additional crystalline layer containing ^{60}Co gamma rays.

Our Fear Confirmed:

The polarization of the thick ^{60}Co γ-ray source was obtained with no difficulty. But we had no such luck for the thin surface ^{60}Co source. The polarization lasted no more than a few seconds, then completely disappeared. What we had feared all along finally happened: the polarization of a thin layer on the surface did not last long enough for actual observation. The reason for this disappearance of nuclear polarization on the surface was probably due to its sudden rise in temperature caused by heat that reached the surface of the specimen by means of radiation conduction or condensation of the He-exchange gas. The only remedy was to shield the thin CMN crystal in a cooled CMN housing. But where could one obtain many large single CMN crystals in a hurry? I decided to return to Pupin Laboratory at Columbia University and try to find ways to grow some CMN crystals.

Beautiful Sight of Those Large Single CMN Crystals:

Purely relying on ingenuity, determination and luck, three of us (an enthusiastic chemist, Herman Fleishman, a dedicated student, Ms. Marion Biavatï, and I worked together uninterruptedly to grow about ten large, perfect, translucent CMN single crystals at the end of three weeks. The day I carried these precious crystals with me back to Washington, I was the happiest and proudest person in the whole wide world.

A schematic diagram showing the demagnetization cryostat used in the measurement of the angular distribution of the electrons from the β-decay of oriented ^{60}Co nuclei is in Fig. 14. The ^{60}Co nuclei were polarized parallel to the axis of the cylindrical cryostat. The electrons were detected by an anthracene scintillation counter. Two NaI γ-ray scintillation counters are also shown. The CMN crystal is known to have a highly anisotropic g-values: $g_{\perp} > g_{/\!/}$. In making the housing, one must line up the crystal axis perpendicular to the demagnetization field and glue the CMN pieces together. (see Fig. 14). Dr. Ambler applied the DuPont cement as it was frequently used in room temperature. This time, we indeed saw an unmistakenly asymmetrical effect on the counting rates when the polarization field was turned on. However, the effect was not only eminently clear but also irreversible! The counting rates never returned to their original values even when the source was warmed up. The shielding CMN

house had caved in!

Genuine Asymmetry Effect Observed:

The second time the housing was put together, very fine nylon threads were used to tie the pieces together. For the first time, we finally saw a genuine asymmetry which coincided exactly with the γ-ray anistropy effect (see Fig. 15). I remember the mood then was more cautious and subdued. The discovery would be big if our observation was real. We cautioned ourselves that more rigorous experimental checks must be carried out before announcing our results to outsiders.

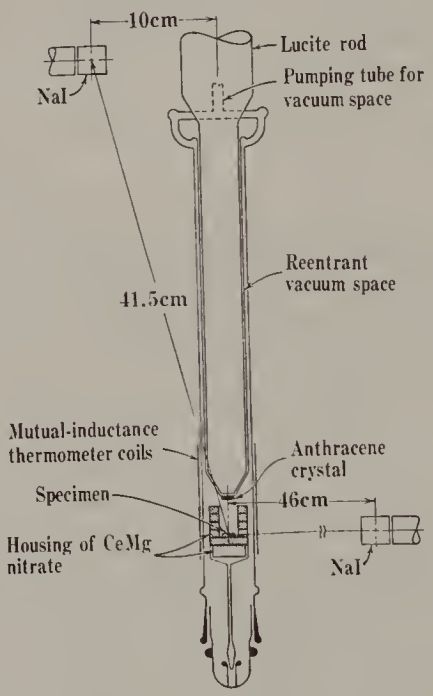

Fig. 14 A schematic diagram showing the demagnetization cryostat used in the measurement of the angular distribution of the electrons from the β-decay of oriented ^{60}Co nuclei. The ^{60}Co nuclei were polarized parallel to the axis of the cylindrical cryostat. The electrons were detected by an anthracene scintillation counter. Two NaI γ-ray scintillation counters are also shown.

The Asymmetry "A" of Beta Decay in Pure (G-T) Transition Compares to That of Beta Decays of Mixed (Fermi and G-T) Interactions:

One day, Prof. Yang in Prof. Lee's office stopped me to ask whether anyone had calculated the interference term between the G-T and Fermi interaction. I told him that Dr. Masato Morita had carried out these calculations in detail and the interference depended on the signs

between C_A and C_V. I said I was pleased that the beta transition in ^{60}Co was a pure G-T transition. We know now that the observed asymmetry parameter A in $^{60}Co(5\rightarrow4)$ $I_z \rightarrow I_f$ is nearly -1, but it is much reduced in mixed transitions such as in Table I; in ^{1}n and ^{19}Ne, the asymmetry parameter A is approximately -10% or less.

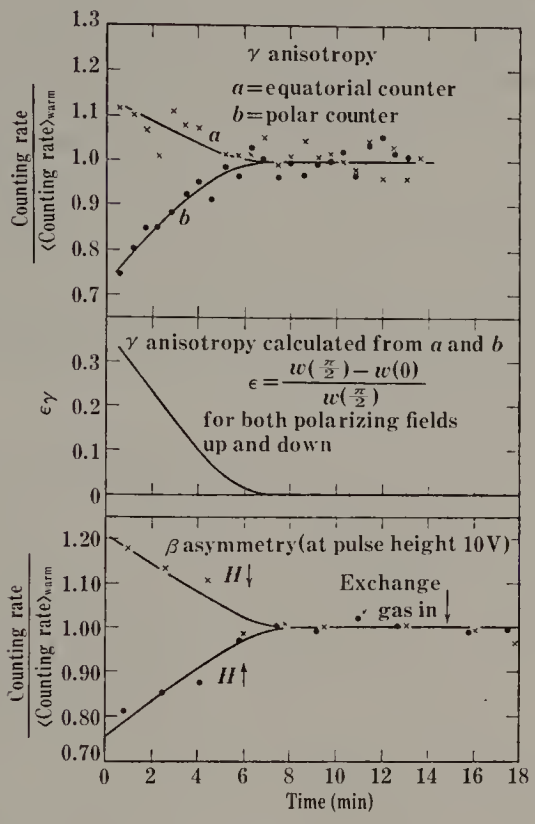

Fig. 15 Results of β asymmetry and γ anisotropy from polarized ^{60}Co experiment. The disappearance of the β asymmetry coincides exactly with the time of disappearance of γ anisotropy. The measured asymmetry indicates that the emission of electrons is preferred in the direction opposite to that of the nuclear spin.

The result of A = -1 was the first indication that the interference between parity conserving and parity non-conserving term in G-T interaction Hamiltonian was close to maximum or $C_A = C'_A$. This result is just what one should expect for a two component theory of the neutrino in a pure Gamow-Teller transition. It also implies that in this case, the charge conjugation is also non-invariant. So we have "one stone killed three birds"; the parity is not valid, charge conjugation is not invariant and the

two-component theory of the neutrino!

Confronted by the clear evidence of the two-component theory of the neutrino, Dr. Lee was pleased and said that this was very good. He then told me that during the summer of 1956 when he and Yang worked together on the parity problem, they had not only entertained the idea of the two-component theory of the neutrino, but had also worked out some details of the theory. However, they felt it was too rash to publish it before the non-conservation of parity in beta-decay was observed and confirmed. When we had the strong evidence of the two-component theory of the neutrino in the ^{60}Co, Dr. Lee and I immediately discussed possible experiments such as the measurement of electron polarization, and the $\pi \rightarrow \mu \rightarrow e$ parity experiments.

The correct interpretation of our very first pioneer experiment on parity non-conservation had a decisive effect. It also suggested the combined "CP" invariance. This combined operation CP was examined by Landau, Wick and Yang even before the Parity Conservation was overthrown.

Laws of Parity Overthrown:

The atmosphere during the period between January 2nd and January 8th 1957 was probably the most tense in our whole experimental venture. Our cryostat at the NBS was made of glass and the glass joints were put together with low temperature vacuum grease which was concocted by melting together glycerine and Palmolive soap (later on we changed to Ivory soap). The trouble which plagued us repeatedly was the super-fluid leak below the lambda point ($T = 2.3K$). Each time this happened it took at least 6-8 hours to warm up, regrease and then cool the cryostat down again. To save time, Hopper slept on the floor in a sleeping bag near the cryostat. Whenever the cryostat reached liquid helium temperature, he would telephone each of us to go to the lab, no matter what time of the night it was. Some winter nights in Washington were very severe indeed!

Toasting The Overthrow of the Law of Parity:

After we had finished all the experimental checks which we had set out to do, we finally gathered together at 2 o'clock in the morning on the morning of January 9th to celebrate the great event. Dr. Hudson smilingly opened his drawer and pulled out a bottle of wine which turned out to be a Chateau Lafite-Rothschild, Vintage 1949. He put it on the table with a few small paper cups. We finally drank to the overthrow of the Law of Parity.

I vividly remember several research workers in other sections of the low temperature laboratories stopping by our lab the next morning and being surprised by the silent and relaxed atmosphere. They suddenly turned around to take a look at our waste paper basket and saw the champagne

bottle, nodded to themselves and murmured among themselves "all right, the law of parity in beta decay is dead".

On the afternoon of January 15th, the Department of Physics at Columbia University called a press conference. Prof. I.I. Rabi announced the dramatic overthrow of a basic law of physics known as the conservation of parity to the public. The next day, the New York Times carried as a front page headline: "Basic Concept in Physics Reported Upset in Tests". The news burst into public view and quickly spread around the world. As Prof. O.R. Frisch of Cambridge University described it in a talk at that time, "The obscure phrase 'parity is not conserved' circled the globe like a new gospel".

The sudden liberation of our thinking on the basic laws of the physical world was overwhelming. Activities along these lines advanced at an unprecedented pace. First, the non-conservation of parity was also observed in the $\pi^{\pm} \rightarrow \mu^{\pm} \rightarrow e^{\pm}$ decays and other weak interactions not restricted to nuclear beta decays. Thus the parity non-conservation is a fundamental characteristic of the weak interactions and soon the Universal Fermi CVC was also proved. The weak interaction has since manifested into one of the four fundamental interactions in Nature known as electro-weak interaction. We were extremely fortunate to have had the opportunity to join in this great venture! These were moments of exhilaration and ecstasy! A glimpse of this wonder can be the reward of a lifetime. Could it be that excitement and ennobling feelings like these have kept us scientists marching forward forever!

Y. Nambu, J. Lee, F. Dyson

V. Telegdi

IMPROMPTU INTRODUCTORY REMARKS

V. L. Telegdi

Swiss Federal Institute of Technology, Zurich

As you can conclude from what you just heard, the Columbia people are still living in the past.* Actually, if they were living in the past I wouldn't be here today, as some of you may know! Well, as they say, time heals all wounds--but it also sometimes wounds old heels... Anyway, there are just a couple of remarks I would like to make, some about T.D. whose birthday we are celebrating today. We all wish him of course many, many more years of health and of creativity. As you know, T.D. Lee has had very many creative ideas, but not all of them turned out to be right. This I once described as "T.D. Lee is smarter than Nature." Well, another fact about him is one that Professor Wu did not, I think, make as clear as it could be, namely: Why was the cobalt experiment done in the U.S. and by that famous collaboration between Columbia and the Bureau of Standards? Why wasn't it done in the place where Dr. Ambler was imported from, in Oxford? Why wasn't it done where all of low temperature was invented, in the Netherlands? And the reason it wasn't done in those places is that there was no T.D. Lee-like person there to provide the push. I think that's the fact that should be remembered--that the time was right, but the place was not right, in those other laboratories. It should be said, for instance, that just before parity violation somebody in Holland wrote an enormous theoretical paper on angular distributions from oriented nuclei (in fact I know who it was, namely Tolhoek). But the proper background in weak interactions was generally lacking in Europe.

The next point is to talk about my old friend, Dick Garwin. It's a friendship that began in 1951 when I joined the University of Chicago. I would say it didn't start out as a friendship the first few weeks I met him. I used to call Dick then "the prince of gratuitous advice". Well, about three months after I got to Chicago he gave me a small piece of advice that wasn't quite right--and after that we became very good friends. What is more remarkable, we also stayed friends after January 1957.

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*Speaker's affiliation was orally announced as "The University of Chicago".

In 1959, we both went to CERN where we worked together on the anomalous magnetic moment of the muon. My financial support came from the National Science Foundation; I remember to this day what I wrote on the application form: "As long as we are in the same country, Dick Garwin and I have to compete. Perhaps at CERN we can collaborate." And in fact we did. It was a very trying time for me because of course no matter what I would like to involve myself in, Dick knew how to do it better. When the experiment was done, I confessed to Dick that I had had a great deal of apprehension working together with him. And he just said one sentence: "Anytime you want a job with IBM, you can have it." So I turn you over to Dick Garwin. By the way--one last remark--he's part Hungarian.

R. Garwin

DEMONSTRATION OF PARITY NONCONSERVATION IN THE $\pi - \mu - e$ CHAIN

Richard L. Garwin

IBM T.J. Watson Research Center

I am very pleased to be here, to have the opportunity to talk about this work of almost 30 years ago, and especially on this happy occasion to celebrate T.D. Lee's sixtieth birthday. We first met in 1947 at the University of Chicago; we were students together, took the same examination and have been friends ever since. If I said all the good things I know about Lee, or even about Telegdi, who has introduced my talk, I wouldn't have time for my talk.

In asking me to recount this work, people were very clever, knowing that they would get the purest possible account of what happened 30 years ago uncontaminated by any subsequent knowledge of particle physics. It's like going to Appalachia to find out about the English language. But in talking about our work this time, I don't need to persuade you of the correctness of the results of our experiment or of its implications; rather I will discuss from the point of view of the experimenter exactly where we started and what we did. Perhaps this will be useful for anyone who thinks that particles announce themselves by their identity, location, and internal parameters directly onto digital tape. I'm privileged that the background for my talk has been provided by Professor Wu and also by Mel Schwartz, so I need only jump in and say what we did. Similarly, when we did this experiment in January 1957, the background, equipment, and knowledge had been provided by others, or by ourselves at other times. So we had only a little bit to do and we could do it reasonably quickly.

In their famous papers of summer 1956 Lee and Yang demonstrated that there was no evidence that would read on

parity nonconservation in the weak interaction. They proposed two experiments. One was the Co^{60} experiment, on which Professor Wu has already reported: the asymmetry of the electron population in the decay of oriented Co^{60}-- the correlation between the average momentum of the distribution, <P>, and the axial vector representing the spin of the nucleus. The other experiment involved the pi-mu-e decay, in which the pion (spherically symmetric) would decay into muons with equal probability in any direction, but the muon would be polarized along its velocity vector, and the spin of the muon would then lead to an asymmetry in the electron distribution. All over the world, people were thinking how to do this experiment; even at Columbia we were thinking how to do this experiment. But it seemed a fiendishly difficult experiment, because if you allow a positive pion created any place to stop-- and nuclear emulsion is where most pions seemed to end up, I don't know why that was-- they decay in 20 nanoseconds or so, giving rise to a muon. But the muon is separated in position by less than a millimeter from the stopping point of the pion. So one could find the stopping pions, find the decay muons, and correlate with each muon track the direction of the decay electron from that muon.

It's daunting because nobody knew, of course, whether parity was conserved or not. There was strong prejudice that it was, and if it was not conserved maybe it was by only a little bit, so that you could frighten yourself with two small parameters ε and d. You had ε perhaps for the nonconservation of parity-- that is for the polarization of the muon-- and another small number (maybe also ε) for the asymmetry of the electron distribution. And there was the problem that only a fraction d of the muon polarization might be retained in its travel through nuclear emulsion or whatever, and d might also be small, so the electron asymmetry with respect to the direction of muon emission might be small like $d\varepsilon^2$. The number of measurements that one would have to make in order to get any statistical significance is then $(1/d\varepsilon^2)^2$, and that might be a very big number indeed. That would lead one to the problem of

avoiding an overwhelming scanning bias in following all of these events. So although people began such experiments, I don't think they had a lot of confidence that they were going to get a result that could be believed.

That's more or less how things stood on the pi-mu-e scene from the summer of 1956 until January 4, 1957. But at Columbia we had some inside information-- not on pi-mu-e, but on what was happening with the confidence in nonconservation of parity. That's because Professor Wu would return from the Bureau of Standards every once in awhile and tell us of her results and her troubles. I was involved in other things at the time; in fact, I was directing a superconducting computer project for IBM. I had left high-energy physics in 1952 after having done a little bit of work and a lot more instrumentation in the development of coincidence circuits and other equipment and techniques, but I was involved at the luncheon table at Columbia and at the seminars.

When I returned from Poughkeepsie Friday evening, January 4, 1957, Leon Lederman telephoned and said "I have an idea". After dinner at home, I met him at the Nevis cyclotron, which is about 15 minutes from my house and we talked. His idea was that it might not be necessary to track each muon from its parent pion, but that there was a way to <u>separate</u> polarized muons from the pions. In particular, the muons from the cyclotron that we had been studying for many years might already be polarized very substantially by virtue of the way in which they were born and collected. So the idea was that if we stopped <u>those</u> muons, and if they would retain their polarization in stopping if they were polarized to begin with, then we wouldn't need to determine the polarization of each muon; we could determine the population-average emission rate of electrons vs. angle from the muon beam. I cannot really explain to you sufficiently how ignorant we all were about these things at that time. We didn't know the spin of the muon; a lot of people who knew particle physics didn't know solid-state physics (of course that's all changed now); a lot of people who knew solid-state

physics didn't know particle physics, and so the scene was ripe for these ideas. In fact, there were two insights, which is why our simple experiment had not previously been done.

Let me explain Leon's idea. Start with a typical pion beam. These were first created at the Berkeley synchrocyclotron, and then at Chicago and Nevis and at every other such synchrocyclotron. The cyclotron contains a large region of intense magnetic field, 18 kilogauss, some 15 feet in diameter. Protons spiral out from the center in the horizontal median plane and strike a target near the periphery. Some 20 or 50 watts of dissipation or nuclear interactions occur in the target, which can be a few mm thick. Mesons-- π^+, π^-, and π^o-- are produced in the strong interaction. They are produced with lower momentum than the proton (having both lower energy and lower mass). The proton beam cannot penetrate too far into the fringing field because although the beam is focused vertically it becomes unstable radially, but one brings it out just about as far as possible, so that it grazes a target. Pions created at the target can enter a particular channel through the 8-ft-thick shielding wall only if they have a particular momentum and initial direction. For many years people had been studying the properties of pions and even of muons from that particular channel.

What is the origin of the muons? Of course they're not created in the nuclear interaction, but from the decay in flight of the pions. In the pion lifetime of 20 nanoseconds or so, a pion travelling at nearly the velocity of light will go 20 feet or 6 feet, and a small fraction of them decay within a few centimeters of the source where they're produced. So from the channel in the shielding wall emerges a mixed beam of pions and muons which have been momentum analyzed (same charge, same momentum) by the fringing field of the cyclotron; they emerge into the experimental hall where the experimenter makes Ph.D. theses out of them.

So Leon's idea was this-- that every muon is some pion's daughter; what can we tell about the parent pions? They are those pions which give a muon of a high enough momentum to get through that channel. Each is a pion which has decayed into a muon forward-- the muon goes forward in flight and not backward--because even though the muon recoil energy is quite low in the pion rest frame, just a few MeV, the muon velocity in the rest frame is not so low because it goes as the square root of the energy. So in the lab frame there is a big difference between the energy of a muon formed from forward decay of a pion to a muon and the energy of a muon arising from backward decay. There are many more pions of the relatively low energy for which forward decay would contribute muons to the particular momentum channel we used, than there are pions of much higher energy for which backward decay could supply muons of the same momentum-- hence the origin of the polarization.

Now for the second insight. When Leon and I met at the cyclotron the question was how to determine the asymmetry in the <u>electron</u> population from these muons which we would stop. If we did find a big asymmetry (and if it were not experimental error) then we would have shown that the muon is polarized, that parity is nonconserved because the muon is polarized and also that the electron distribution from the decay of polarized muons is nonsymmetric. Well, that's pretty tough because you have to stop the muon in a block of something which is a couple centimeters thick (the muons have a distribution in momentum and hence in range); then when you measure the number of electrons emitted forward vs. the number backward there is scattering and energy loss and all sorts of things, not to mention a 20-gauss fringing field of the cyclotron itself, which would cause the muon spin to precess so long as the muon lived. Even an isotropic angular distribution of emitted electrons could acquire a front-back asymmetry because of these "dirt effects."

We had a second insight. For some years I had been working on nuclear magnetic resonance of liquid and solid He^3. The

second idea was not to swing the counters around to sample the electron distribution from the muon decay but to swing the decay distribution itself around into fixed counters. This was a reasonable idea for two reasons. First, because it was a lot easier-- we didn't have any swinging counters. Second, because if you could swing the muon spin you wouldn't change at all the relationship between the counters and the distribution of stopping muons, so a lot of these experimental errors would not occur. We thought we would swing this distribution (which if the muon had spin 1/2, had maximum complexity 1 + Acosθ) past some fixed counters. Now the question is how to do that. The 85-MeV pions have range of about 5 inches in graphite, so we used an 8-inch graphite block to stop all the pions. The muons of the same momentum have a longer range and emerge from the block with about 1-cm residual range. The muon stopping rate is maximized in a rather thin block of appropriate material, and the idea would be to swing an electron detecting telescope to detect the (presumed) asymmetric distribution of decay electrons. In principle, one need only expose photographic plates and see where the electron density is highest; but that won't work, because the background in the room is too great. One looks at the electrons really by coincidence in two counters with a substantial amount of material between, to insist that the electrons have at least 10 or 20 MeV energy. One has counters on the input beam as well, so that one looks not at any time but only in coincidence with the stopping muon. Even with coincidence-counting selection of electrons accompanying stopping muons, the swinging counter telescope is a bad experiment because of all of the problems of asymmetry of stopping of the muon that you might expect. We made a better experiment.

We started at 8 pm at the cyclotron and walked through the machine shop-- I always walk through machine shops when I want an idea-- and in the machine shop waiting to be picked up was a cylinder of Lucite which only needed cutting in the hacksaw and putting in the lathe and winding a coil on it to produce Z-axis magnetic field to precess these muons.

Electrons precess at 2.8 megahertz per gauss; muons are 207 times as heavy. If they have the same g value, (which nobody knew) they would precess at 1/207 of that rate or about 14 kilohertz per gauss. Since muons (at rest) live only 2.2 microseconds, if we're going to do anything to them we have to do it in a microsecond or so. You would like to have them precess half a revolution or so, so you're talking about 40 gauss magnetic fields which can be produced without overheating a thin layer of copper wire, and so the Lucite coil form seemed reasonable.

The idea then was to stop the muons, have a fixed counter telescope, precess the muon spin population (if there was any net population-average spin), and see whether there was a variation (with time after muon stop) in electron counting rate in the fixed electron telescope. To eliminate the influence of the 20 gauss fringing field of the cyclotron, we bent up some mu metal-- it has nothing to do with mu mesons, sorry about that-- to shunt the magnetic field of the cyclotron so that it did not affect the muon spins. It turns out that cyclotron fringing fields had been affecting muon spins for a long time and that's why Marcel Weinrich was still around doing his experiment on lifetime of negative muons stopping in matter. Figure 0.1 shows the idea in classical physics terms and in cartoon form, while Figure 0.2 is somewhat more quantitative.

We started running Friday night. We had to do this all remotely from the counting room about 100 meters away from the experimental hall. We did things which were not strictly routine; we disconnected some of the counting signal leads and put current through those leads so that we could vary the current from the experimenter's room in order to precess the muons more or less. Of course we had to choose the material for stopping the muon. I had written some papers on decay of positrons in metals and, obviously (when you think about it for a moment) the only thing that affects a spin one-half is a magnetic field; if you want the spin to retain its orientation as it comes to rest and stays in the material,

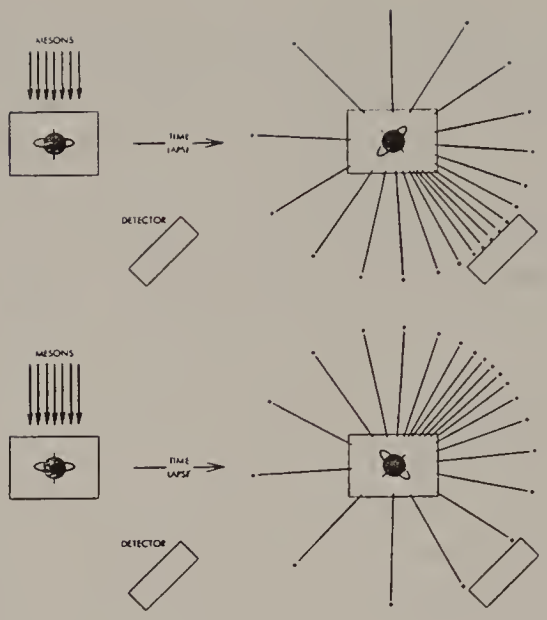

Fig. 0.1 Cartoon of precession experiment. View of horizontal plane, with muon beam entering at the top of frame. Muons stop in block at left, where they experience a steady vertical magnetic field. After a time lapse, a muon decays with electron emission probability vs. angle shown by the density of radial strokes. In the presence of magnetic field and time lapse, the asymmetric decay distribution precesses and gives rise to time variation of electrons into the detector.

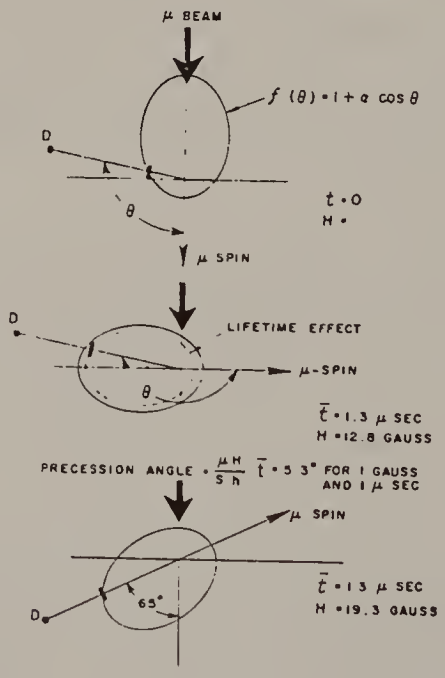

Fig. 0.2 Decay distribution of Fig. 0.1 is depicted as a polar plot, with precession in time and magnetic field indicated.

you want to free it from interaction with free or bound electrons that have big magnetic fields, at least from electrons with correlation times of the order of microseconds. Graphite seemed reasonable since we had it around, and it is a nearly metallic, low-Z material. It was a fortunate choice.

The cyclotron normally shut off early Saturday morning for weekends, and this time it was going to be off also on Monday for maintenance. By Saturday morning we had been counting electrons in a fixed counter in a certain time interval after muon stop, and we had a big variation with precession current. But by the time the cyclotron shut off the effect had gone away; somewhat disconsolate we went down to the experimental area to turn off the counters, and we found that the cylindrical coil form had all of its coils lying at its base. In our exuberance we had turned on the current in the coil too rapidly; the copper expanded faster than the Lucite and fell to the bottom. We didn't know whether to be happy or sad.

That was our first experiment, January 4-5. We knew one reason why it had failed but we weren't sure it had succeeded. Monday, when we couldn't work at the cyclotron, we came to the campus and went around to see whether people had Helmholtz coils to produce a uniform field, to substitute for our hand-built solenoid. But it was much more work to use the Helmholtz coil than to make a new arrangement. So by the time the cyclotron started working again Monday night we had simplified and eliminated. This time we wound the coil this time right on the block of graphite. Who ever said a solenoidal coil with the uniform magnetic field inside which is obtained from an infinite cylinder has to be obtained from a <u>circular</u> cylinder? So this was a rectangular cylinder. We made the shield serve double duty; it not only shunted the external magnetic field of the cyclotron, but it kept the solenoid field from fringing and made the field uniform in the carbon. We had a famous coffee can which I think is now

in the Smithsonian to support our carbon block without too much neighboring mass.

Monday evening, January 7, we began to take data with the apparatus which is sketched in Fig. 1 of the accompanying Physical Review publication of February 15, 1957. Later, of course, there were many more experiments done here and elsewhere on parity nonconservation and the effects that one could measure. The "pion channel" was normally used for an 85-MeV pion beam. We stopped all the pions in 8 inches of graphite. The input coincidence telescope (counters 1 and 2) selected muons stopping in the graphite wound with our precession solenoid. The electron telescope (counters 3 and 4) detected decay electrons. We could require a minimum energy of the emitted electrons by the amount of graphite in the telescope. The mu-metal magnetic shield is indicated. So that is the experimental setup. Just to recount why we were using that apparatus, here is what we expected to do: Muons come in, with a spin polarization, on average, for instance in the forward direction. After a certain lapse of time the orientation of the spin on the average would have precessed in the magnetic field and a certain counting rate would be observed in the electron detector. But if we had waited a longer time or if we had had a bigger magnetic field, there would be more precession and the counting rate would be different. And so we were hoping to detect a variation in the electron counting rate at fixed angle, as a function of time and magnetic field; such a "precession curve," would be the signal of polarized muons with nonuniform electron distribution.

By Tuesday morning we had the result of Fig. 2. Small currents of 0.2 ampere brought us to a peak in the counting rate. At -0.18 ampere, there was a minimum. The effect was very big. Relative to the counting rate at zero applied field, the peak was 1.3 and the minimum 0.7-- a peak-to-valley ratio of almost 2, which made even the photographic emulsion measurements possible if one had known

(although the muon polarization is less in photographic emulsion).

Slightly more quantitatively, as shown in Fig. 0.2, at time zero there is the electron distribution $1 + A\cos\theta$. At an average decay time of 1.3 microseconds in a field of 12 gauss the population would be rotated this much; and at a later time, 19 gauss, or a bigger field 1.3 microsecond, it would be rotated further. In order to determine the direction of rotation we moved the counter physically to see which field brought the same counting rate, because we were interested also not only in the magnitude of the asymmetry and rotation, but also in their signs.

This work was done by Leon Lederman and Marcel Weinrich and myself. Shortly after, other work at Nevis -- on energy dependence of electron asymmetry and resonance experiments-- was done with Shelley Penman and Allan Sachs. It didn't take long before we really tortured those muons, not just with steady magnetic fields but by putting tens of kilovolts of pulses on them so that we could measure their magnetic moment more accurately.

By Tuesday, January 8, this first experiment was complete. In this paper from the Physical Review we pointed out that we had, by virtue of a good deal of luck, demonstrated a considerable number of things. First a large asymmetry. Second the $1 + A\cos\theta$ form of the asymmetry; therefore from the guiding theory not only was parity not conserved, but charge conjugation was violated itself.

The g value of the muon was 2.0, within 5%, which we knew in two independent ways. One was the precession rate and the other from the orientation of the polarization when the muon stopped. We knew that their momentum had been turned some 90° in the fringing field, and the observation showed that their spin had been turned by about the same amount. Even relativistically for g value of 2, the spin and the momentum remain coupled, so we knew g was close to 2. Of course we

had immediately the idea of looking at the deviation of g from 2-- a classic experiment done on electrons at Columbia University-- and we wanted to do it for the muon as well.

Negative muons stopped in carbon cascade very rapidly to the K shell of muonic carbon. They still show an electron-decay asymmetry of about 5%; so the μ^- polarization in the K-shell is about 15% of that of the positive muons. We had no doubt that the polarization of μ^- at birth was very much the same as that of μ^+, so the depolarization was an atomic effect, as could be readily imagined. By the time this paper was published we had also observed the energy dependence, not very accurately, for comparison with the predicted two-component neutrino theory; the peak-to-valley ratio for different energy electrons ranging from 1.8 to 2.2 or so. The suggestion that the μ^+ could be a useful probe of the space and time variation of magnetic fields on an an atomic scale has been borne out by the modest industry of "muon spin rotation" measurements.

This first experiment took us 4 days-- of course after people had been building cyclotrons, and making theories, and doing experiments, and accumulating experimental apparatus for a long time. Our experiment laid the groundwork for other experiments. I remember Jim Rainwater asking "Are you going to persist in these resonance measurements in which you apply radiofrequency pulses to the muons?" which isn't so easy because you only have the 2 microseconds. Nobody had previously affected a muon before it had died. You have to do this in real time; you have to form the coincidences, apply the pulse, and precess the spin. I could tell from the way he said this that he had something else in mind. So I told him that I was going to persist in this. I think he gave me three days to devise a better way which, of course, he had already thought of; that is a correlation measurement in which one does not apply a pulse to determine the resonance curve for flipping the time-average muon spin population, but instead allows the muon to precess very rapidly in a fixed magnetic field, and correlate with a

reference oscillator the electron detections in a fixed telescope. We made at least two experiments like that. I beat his deadline so Jim Rainwater didn't do the experiment. We did the experiment, Allan Sachs and Shelley Penman and the rest of us.

I thank Tsung-Dao for thinking of this very fine theory so that we could have the great fun of doing an experiment which required almost no work and gave us such great enjoyment. Thank you.

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Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon*

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LEE and Yang[1–3] have proposed that the long held space-time principles of invariance under charge conjugation, time reversal, and space reflection (parity) are violated by the "weak" interactions responsible for decay of nuclei, mesons, and strange particles. Their hypothesis, born out of the $\tau-\theta$ puzzle,[4] was accompanied by the suggestion that confirmation should be sought (among other places) in the study of the successive reactions

$$\pi^+ \rightarrow \mu^+ + \nu, \tag{1}$$

$$\mu^+ \rightarrow e^+ + 2\nu. \tag{2}$$

They have pointed out that parity nonconservation implies a polarization of the spin of the muon emitted from stopped pions in (1) along the direction of motion and that furthermore, the angular distribution of electrons in (2) should serve as an analyzer for the muon polarization. They also point out that the longitudinal polarization of the muons offers a natural way of determining the magnetic moment.[5] Confirmation of this proposal in the form of preliminary results on β decay of oriented nuclei by Wu *et al.* reached us before this experiment was begun.[6]

By stopping, in carbon, the μ^+ beam formed by forward decay in flight of π^+ mesons inside the cyclotron, we have performed the meson experiment, which establishes the following facts:

I. A large asymmetry is found for the electrons in (2), establishing that our μ^+ beam is strongly polarized.

II. The angular distribution of the electrons is given by $1+a\cos\theta$, where θ is measured from the velocity vector of the incident μ's. We find $a=-\frac{1}{3}$ with an estimated error of 10%.

III. In reactions (1) and (2), parity is not conserved.

IV. By a theorem of Lee, Oehne, and Yang,[2] the observed asymmetry proves that invariance under charge conjugation is violated.

V. The g value (ratio of magnetic moment to spin) for the (free) μ^+ particle is found to be $+2.00\pm0.10$.

VI. The measured g value and the angular distribution in (2) lead to the very strong probability that the spin of the μ^+ is $\frac{1}{2}$.[7]

VII. The energy dependence of the observed asymmetry is not strong.

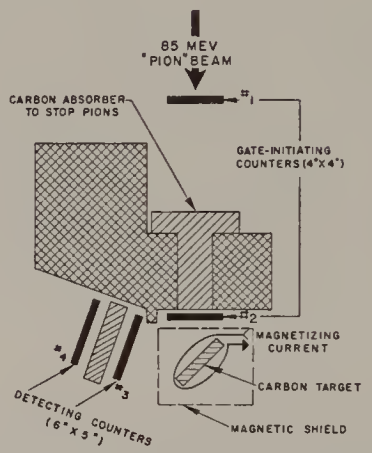

FIG. 1. Experimental arrangement. The magnetizing coil was close wound directly on the carbon to provide a uniform vertical field of 79 gauss per ampere.

VIII. Negative muons stopped in carbon show an asymmetry (also leaked backwards) of $a\sim-1/20$, i.e., about 15% of that for μ^+.

IX. The magnetic moment of the μ^-, bound in carbon, is found to be negative and agrees within limited accuracy with that of the μ^+.[8]

X. Large asymmetries are found for the e^+ from polarized μ^+ beams stopped in polyethylene and calcium. Nuclear emulsion (as a target in Fig. 1) yields an asymmetry of about half that observed in carbon.

The experimental arrangement is shown in Fig. 1. The meson beam is extracted from the Nevis cyclotron in the conventional manner, undergoing about 120° of magnetic deflection in the cyclotron fringing field and about −30° of deflection and mild focusing upon emerging from the 8-ft shielding wall. The positive beam contains about 10% of muons which originate principally in the vicinity of the cyclotron target by pion decay-in-flight. Eight inches of carbon are used in the entrance telescope to separate the muons, the mean range of the "85-"Mev pions being ~5 in. of carbon. This arrangement brings a maximum number of muons to rest in the carbon target. The stopping of

a muon is signalled by a fast 1–2 coincidence count. The subsequent beta decay of the muon is detected by the electron telescope 3–4 which normally requires a particle of range >8 g/cm^2($\sim$25-Mev electrons) to register. This arrangement has been used to measure the lifetimes of μ^+ and μ^- mesons in a vast number of elements.[9] Counting rates are normally $\sim$20 electrons/min in the μ^+ beam and $\sim$150 electrons/min in the μ^- beam with background of the order of 1 count/min.

In the present investigation, the 1–2 pulse initiates a gate of duration $T=1.25$ μsec. This gate is delayed by $t_1=0.75$ μsec and placed in coincidence with the electron detector. Thus the system counts electrons of energy >25 Mev which are born between 0.75 and 2.0 μsec after the muon has come to rest in carbon. Consider now the possibility that the muons are created in reaction (1) with large polarization in the direction of motion. If the gyromagnetic ratio is 2.0, these will maintain their polarization throughout the trajectory. Assume now that the processes of slowing down, stopping, and the microsecond of waiting do not depolarize the muons. In this case, the electrons emitted from the target may have an angular asymmetry about the polarization direction, e.g., for spin $\frac{1}{2}$ of the form $1+a\cos\theta$. In the absence of any vertical magnetic field, the counter system will sample this distribution at $\theta=100°$. We now apply a small vertical field in the magnetically shielded enclosure about the target, which

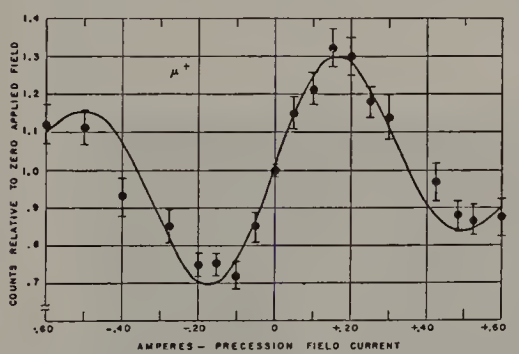

FIG. 2. Variation of gated 3–4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution $1-\frac{1}{3}\cos\theta$, with counter and gate-width resolution folded in.

causes the muons to precess at a rate of $(\mu/s\hbar)H$ radians per sec. The probability distribution in angle is carried around with the μ-spin. In this manner we can, with a fixed counter system, sample the entire distribution by plotting counts as a function of magnetizing current for a given time delay. A typical run is shown in Fig. 2. As an example of a systematic check, we have reduced the absorber in the telescope to 5 in. so that the end-of-range of the main pion beam occurred at the carbon target. The electron rate rose accordingly by a factor of 10, indicating that now electrons were arising from muons isotropically emitted by pions at rest in the carbon. No variation in counting rate with magnetizing current was then observed, the ratio of the rate for $I=+0.170$ amp to that for $I=-0.150$ amp, for example, being 0.989 ± 0.028. The highest field produced at the target was $\sim$50 gauss which generates a stray field outside of the magnetic shield of $<\frac{1}{10}$ the cyclotron fringing field of 20 gauss. The only conceivable effect of the magnetizing current is the precession of muon spins and we are, therefore, led to conclusions I–IV as necessary consequences of these observations.

The solid curve in Fig. 2 is a theoretical fit to a distribution $1-\frac{1}{3}\cos\theta$, where

(1) the gyromagnetic ratio is taken to be $+2.00$;[10]

(2) the angular breadth of the electron telescope and the gate-width smearing are folded in, as well as (to first order) the exponential decay rate of muons within the gate;

(3) the small residual cyclotron stray field (*up* for Fig. 2, the positive magnetizing current producing a *down* field) is included. This has the accidental effect of converting the 100° initial angle ($H=0$) to 89° as in Fig. 2. We note that this experiment establishes only a lower limit to the magnitude of a, since the percent polarization at the time of decay is not known. If polarization is complete, $a=-0.33\pm0.03$.

Proof of the 2π symmetry of the distribution and the sign of the moment was obtained by shifting the electron counters to 65° with respect to the incident muon direction. The repetition of a magnetizing run yielded a curve as in Fig. 2 but shifted to the right by 0.075 ampere (5.9 gauss) corresponding to a precession angle of 37°, in agreement with the spatial rotation of the counter system. Thus we are led to conclusions V and VI.

A specific model, the two-component neutrino theory, has been proposed by Lee and Yang[3] in an attempt to introduce parity nonconservation naturally into elementary particle theory. This theory predicts, for our experimental arrangement and on the basis of 1.86 for the integrated spectrum (Fig. 2), a ratio of the order of 2.5 for energies greater than 35 Mev. We have increased the amount of absorber in the electron telescope to exclude electrons of less than $\sim$35 Mev. The resulting peak-to-valley ratio was then observed to be 1.92 ± 0.19.[11]

We have also detected asymmetry in negative muon decay and have verified that the moment is negative and roughly equal to that of the positive muon.[7] The asymmetry in this case is also peaked backwards.

Various other materials were investigated for μ^+ mesons. Nuclear emulsion as a target was found to have a significantly weaker asymmetry (peak-to-valley ratio of 1.40 ± 0.07) and it is interesting to note that this did not increase with reduced delay and gate width. Neither was there any evidence for an altered moment. It seems possible that polarized positive and negative muons will become a powerful tool for exploring magnetic fields in

nuclei (even in Pb, 2% of the μ^- decay into electrons[9]), atoms, and interatomic regions.

The authors wish to acknowledge the essential role of Professor Tsung-Dao Lee in clarifying for us the papers of Lee and Yang. We are also indebted to Professor C. S. Wu[6] for reports of her preliminary results in the Co^{60} experiment which played a crucial part in the Columbia discussions immediately preceding this experiment.

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† Also at International Business Machines, Watson Scientific Laboratories, New York, New York.

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[10] The field interval, ΔH, between peak and valley in Fig. 2 gives the magnetic moment directly by $(\mu\Delta H/s\hbar)(t_1+\frac{1}{2}T)\delta=\pi$, where $\delta=1.06$ is a first-order resolution correction which takes into account the finite gate width and muon lifetime. The 5% uncertainty comes principally from lack of knowledge of the magnetic field in carbnn. Independent evidence that $g=2$ (to ~10%) comes from the coincidence of the polarization axis with the velocity vector of the stopped μ's. This implies that the spin precession frequency is identical to the μ cyclotron frequency during the 90° net magnetic deflection nf the muon beam in transit from the cyclotron to the 1–2 telescope. We have designed a magnetic resonance experiment to determine the magnetic moment to ~0.03%.

[11] *Note added in proof.*—We have now observed an energy dependence of a in the $1+a$ cns θ distributinn which is somewhat less steep but in rough qualitative agreement with that predicted by the two-component neutrino thenry ($\mu\rightarrow e+\nu+\bar{\nu}$) withnut derivative coupling. The peak-to-valley ratios for electrons traversing 9.3 g/cm², 15.6 g/cm², and 19.8 g/cm² of graphite are observed to be 1.80±0.07, 1.84±0.11, and 2.20±0.10, respectively.

B. Hyams, W. A. Wallenmeyer, L. M. Lederman

M. I. Sovern, J. Lee, L. M. Lederman

N. P. Samios

INTRODUCTORY REMARKS

Nicholas P. Samios

Brookhaven National Laboratory

Dick Garwin and Mel Schwartz, in the introductory segment of their talks noted several reasons why we are here today--namely to celebrate the overthrow of parity and T.D.'s 60th birthday. I would like to add a third; namely that of an act of penance due to the fact that many of us didn't listen to T.D. as intently as we should have, and as such delayed unearthing great new physics. As you just heard from Garwin, the Nevis experiment on parity took just three days from idea to result; these ideas were on paper for a much longer time. Mel commented on the search for parity violation in strange particle decays. In fact, that was the subject of my thesis. Unfortunately it took a bit longer than desirable to obtain a definite result. A further ingredient present today is, of course, nostalgia. As such I thought I would comment a little bit about the ambiance at Columbia during those days in the late 50's. In addition to the excitement associated with the many bright physicists present who subsequently became luminaries, there was a great comradery as well as competition. As young instructors and graduate students, many of the senior people, namely Jack Steinberger, would go around calling us "Professor". We felt quite good about this until one day at Nevis when we were walking down the hall, a janitor came by and he also was greeted with a "Hi ya, Professor."

As noted earlier, we are gathered here to honor T.D. who has affected us all at Columbia and nationally. I've had the pleasure of being associated with T.D. as a student and colleague, and in fact a few years ago we taught a joint course together where he covered theory and I the experimental part of particle physics or vice versa; I forget. As you know, T.D. always has a strong interest in both the experimental work and the theory. However, one of his great strengths, that hasn't been commented on, is that he opens up new fields, and that is the sign of a great man. Weak interactions became a great field for 10-20 years, and if you go back and look at the 1959 letter by Lee and Yang on neutrinos, it's all there including the W's and Z's. I remember at the London Conference,

over ten years ago, T.D. commented to me about the complexity of the vacuum, and he kept saying, that's where the next great effort has to go; you note the subsequent great theoretical activity involved in exploring the vacuum, which is not as simple as we thought. In addition, T.D. has very strong opinions, and we've all sat at his feet where he's told us some new idea. Of course he had worked on it for months, and had simplified it. You may have an argument against it but since he has thought about it for some time, you lose. In fact, to characterize T.D. I would say, if God were as smart as T.D. he would do it T.D.'s way. Right?

My next comment concerns strength of personality. In the audience there are two people that I know of--T.D. one and I.I. Rabi the other-- who could be in a committee meeting where the vote is 11 to 1, and 1 wins. I value both these individuals' counsel a great deal in helping me run my democratic regime at Brookhaven.

As you have noticed by now, there is a big connection among chairmen and speakers. It's Columbia-connected. The next speaker is one of this group, Dr. S.C.C. Ting. As you have noticed, it's almost incest. Ting came to Columbia from Michigan, where he worked under Marty Perl (Rabi student). It is ironic that at Columbia he worked with Leon. Sam is the father of what I would call the revolution of '74, where the J was discovered in September of that year. When I heard of it, of course there was great excitement. I, however, was also somewhat saddened by the discovery because it was apparent to me that once there was a fourth quark, there will be five and six, and that I would not be alive to know the whole picture. Three was such a simple number; as well as possessing religious significance, it's got all sorts of things going for it. Unfortunately it's not consistent with nature.

Parenthetically I note that at this time, we had the pleasure of finding the Λ_c charmed baryon in December of 1974 helping to unravel the significance of the September revelations--namely a new quark, charm. There's a thread in Sam's experimental efforts, and I would say the thread is vector mesons. Some of us do our thesis over and over again, but Sam's activity is centered around vector mesons. He had a reputation as a giant killer. His early fame was showing that other people's work was not entirely correct. He did extensive work on rho's, omega's and phi's in photoproduction. Then there was the J, the finding of gluons, and detailed work on QCD. In fact, the story goes "where Sam's foot steps, the grass doesn't grow there anymore." (Sounds better in Greek.) Now, he's the leader of what one would call the world's largest detector, L3. The amazing thing about that is the magnitude and the massive organization involved, an international involvement--which includes people from the U.S.S.R., Mainland China, East Germany, India, Western Europe and the U.S. In fact, at a time when international relations were not that great, Sam flew from the U.S. to Moscow to

pick up five kilograms of germanium oxide, and took it to Shanghai where it was to be made into BGO for the L3 detector. I have a great admiration for a man who could do that. As Mel Schwartz went from high energy physics to the presidency of a company, I am looking forward to the day when Sam becomes president of General Motors. I give you Sam Ting.

N. Samios and S. C. C. Ting

M. Samios (partly hidden), S. C. C. Ting, G. Ekspong, S. Lindenbaum

SOME RECENT EXPERIMENTS IN HIGH ENERGY PHYSICS

Samuel C. C. Ting

Massachusetts Institute of Technology

INTRODUCTION

I first heard of Professor Lee's name and the violation of parity when I was in High School. I first met Professor T.D. Lee in April 1963 when I came to Columbia University to be interviewed for a position of Instructor in the Physics Department. I had done my thesis work on πp scattering at 5 GeV. My results showed that the prediction of Regge theory was not correct at 5 GeV. I was quite excited about my results. T.D. Lee, after listening to me for a few minutes, told me there was not much physics in what I was doing. Nevertheless, I was hired at Columbia.

During my short stay at Columbia, I had the opportunity to observe the ways physics was carried out by T.D. Lee and by some of the great physicists of our time (L. Lederman, M. Schwartz, J. Steinberger, C.S. wu and others). Over the last twenty years, I have had many occasions to have in-depth discussions with T.D. Lee. This is particularly so before I undertake a new experiment and before my publication of an important experimental result. I have benefited greatly from these discussions.

Indeed, T.D. Lee's work has had a most important influence in almost all branches of experimental high energy physics.

On this happy occasion of celebrating Professor Lee's sixtieth birthday, I have selected a few examples of Professor Lee's vast accomplishments to illustrate his many fundamental contributions to experimental high energy physics :

My first example is the question of baryon-number conservation [ref. 1]

1. IS BARYON-NUMBER CONSERVATION A LOCAL GAUGE SYMMETRY ?

Background

Charge conservation : Charged particles must be created in positive-negative pairs. For example, for pions, interactions must consist of terms like $\pi^+\pi^-$; this means there is invariance under global gauge transformations :

$$\pi^+(x) \to e^{iQ\alpha}\pi^+(x)$$

$$\pi^-(x) \to e^{-iQ\alpha}\pi^-(x)$$

α = arbitrary constant, same for all charged particles

Q = charge

One can decide to call a π^+ at x "positively charged", but one must maintain the same convention everywhere in the universe.

We know that in the case of charge the symmetry is in fact larger. There is invariance under local gauge transformation :

$$\pi^+(x) \to e^{iQ\alpha(x)}\pi^+(x)$$

$$\pi^-(x) \to e^{-iQ\alpha(x)}\pi^-(x)$$

$\alpha(x)$ = arbitrary space-time function

This means that we are completely free to define a π^+ as positive here, but negative over there. This requires the existence of a massless vector particle, **the photon**, which keeps track of our assignment of phase at different x.

The enlargement of global to local gauge transformation generates the familiar minimal electromagnetic coupling.

Analogy

We know that baryon number is conserved. This means that baryons can be created only in particle-antiparticle pairs. Assign to baryons a baryonic charge η, and antibaryons, $-\eta$. Then there is global gauge invariance of the type

$$P(x) \rightarrow e^{i\eta\gamma}P(x)$$

$$\bar{P}(x) \rightarrow e^{-i\eta\gamma}\bar{P}(x)$$

γ = arbitrary constant

The question is whether nature also makes this into a local gauge symmetry.

If this were a local gauge symmetry, there would exist a new '**photon**', and a new type of electromagnetic-like forces, except that baryonic charge replaces the electric charge.
We would then expect Coulomb-like repulsion between two massive, electrically neutral bodies. The long-range force, including gravity, would be

$$F = -\frac{GM_1M_2}{R^2} + \frac{\eta^2A_1A_2}{R^2} = -\frac{GM_1M_2}{R^2}\left(1 - \frac{\eta^2A_1A_2}{GM_1M_2}\right)$$

where

R = distance of separation
G = gravitational constant
M = mass
A = nucleon number

Because of nuclear binding, the ratio A/M varies from material to material by 1 part in 10^{00}.

This variation would have shown up in the Eötvös experiment as a deviation between gravitational and inertial mass (Take M_1, A_1 to be those of the Earth. A_2/M_2 varies from material to material by 10^{-3}. Approximately : $M_1 \approx A_1 m_p$) :

$$\frac{M_{grav}}{M_{inert}} \text{ varies by } \frac{\eta^2}{GM_p^2} \times 10^{-3}$$

M_p = Proton mass

The Eötvös experiments established the variation to be less than 10^{-8}
Therefore

$$\frac{\eta^2}{GM_p^2} < 10^{-5}$$

Such a long-range force has not been observed, and it is consistent with experiments to assume that Baryon-number conservation is not a **'gauged'** symmetry.

Thus, baryon-number is not as sacrosanct as charge, and may be only approximately conserved. It is now believed that this explains why the baryon number of the universe can be non-zero today, even though it must have been zero at the big bang.

Experiments :

The study of baryon conservation is one of the most important subjects in particle physics. There are now two independent developments :

The question of existence of intermediate-range forces :

The re-analysis of the Eötvös experiment [ref. 2] by E. Fischbach et al. finds that the Eötvös data are sensitive to the composition of the materials used, and that their results support the existence of an intermediate-range coupling to baryon number or hypercharge. There are now more than twenty experiments world-wide to search for the existence of intermediate-range forces. These experiments are in three groups :

a) Laboratory Eötvös experiments,
b) Eötvös experiments near a mountain,
c) Repetitions of the Galileo experiments.

The question of proton decay :

The new interests on proton decay experiments is a direct consequence of the recent developments in the grand unification theory. The current limit (lifetime x branching ratio) is of the order of 10^{32} years. This is also a world-wide effort with major experiments in the United States (for example the IMB Group), in Italy (for example, the Grand-Sasso experiments), in India and in Japan.

2. QUARKS AND LEPTONS

SEARCH FOR NEW QUARKS

When I first came to Columbia in 1964, Professor Lee raised a very important question on the consequences of integral charged quarks [ref. 3]. Briefly, my understanding is as follows :

Assume two massive quark triplets (under SU(3) flavor) :

Quarks	$(\alpha^0$	α^1	$\alpha^2)$	$(\beta^0$	β^1	$\beta^2)$
charge	q	q+1	q	q	q+1	q
I-spin	singlet	doublet		singlet	doublet	
Baryon number	n (Boson)			n + 1 (Fermion)		

Construct known $[SU(3)]_{flavor}$ multiplets :

$(\bar{\alpha}\alpha)_8$ = Pseudoscalar meson octet

$(\bar{\beta}\beta)_8$ = Vector meson octet

$(\bar{\beta}\beta)_1$ = Vector meson singlet

$(\bar{\alpha}\beta)_8$ = Baryon octet

Assume $[SU(3)]_{flavor}$ violation occurs only through the mass splitting of the quarks. One obtains the following sum-rule :

$$2M_{\varphi}^2 + M_{\omega}^2 + M_{\rho}^2 = 4M_{k^*}^2$$

Experiments :

This work stimulated a series of experiments by L.M. Lederman, W. Lee, myself and others [see ref. 4, 5, 6]. Indeed, to make sure we were able to search for α and β particles effectively at A.G.S. in 1964, we first studied the production of antiprotons via nuclear motion below the production threshold. The antiproton yield shows that at AGS, with

10^{12} protons, we can provide about 10^5 collisions per pulse which have 14-20 GeV available in the center of mass system [ref. 4]. We spent a great deal of efforts to ensure the spectrometer could reach the desired sensitivity and, as a calibration, we observed for the first time the production of antideuterons [ref. 5]. The final experiment set the limits of α and β particles to be produced in the mass range of $3 \leq m \leq 7$ GeV with a production cross section less than 10^{36} cm^2 sr^{-1} (GeV/c)$^{-1}$ per Be nucleus.

PHOTONS AND LEPTONS

I left Columbia in 1965 and spent the next decade in Hamburg, testing quantum electrodynamics and studying the photoproduction and leptonic decay of vector mesons [ref. 7]. I met Professor T.D. Lee in the German Physical Society meeting in Munich, 1967, and spent a very pleasant morning with him. Professor Lee asked me what I was doing and on hearing that I was measuring leptonic decays of vector mesons, he spent some time explaining to me the work he was doing with M. Kroll and B. Zumino [ref. 8] and the precise meaning of vector dominance, which briefly I reconstruct as follows :

PRECISE MEANING OF VECTOR DOMINANCE

T.D. Lee, M. Kroll and B. Zumino showed that 'to a very good approximation the entire hadronic electromagnetic current operator is identical with a linear combination of the known vector meson fields.'

A consistent Lagrangian theory can be constructed on this basis. Renormalization effects from the strong interaction are taken into account.

$$J_\mu^{\text{isovector}} = -\frac{m_\rho^2}{g_\rho}\rho_\mu$$

$$J_\mu^{\text{isoscalar}} = -\frac{1}{2}g_y^{-1}\left[(\cos\Theta_v)\, m_\varphi^2\varphi_\mu - (\sin\Theta_y)m_\omega^2\omega_\mu\right.$$

g_y = a coupling constant

Θ_y = a mixing angle

These parameters are introduced through an analysis of the hypercharge current.

$\Theta_y = 0$ in limit of $(SU(3))_{flavor}$ symmetry.

Different models of $(SU(3))_{flavor}$ violation can be made, based on mass mixing, or on current mixing.

Result : $\frac{g_\rho^2}{4\pi} \approx 2.3$

Input : Rate $(\rho^0 \to \pi^+\pi^-) = (115.5 \pm 8.2)$ MeV

$$\frac{\rho^0 \to \mu^+\mu^-}{\rho^0 \to \pi^+\pi^-} = (4.3 \pm 1.4) \times 10^{-5}$$

They also obtained the following decay rates :

$\Gamma(\varphi \to e^+e^-)$	1.2 (KeV)
$\Gamma(\omega \to e^+e^-)$	0.6 (KeV)

Experiments :

The Table (below) summarizes the twenty years of efforts on the systematic study of lepton decays of vector mesons by my group and by others. The results showed remarkably good agreement with the results T.D. Lee predicted in 1967.

Leptonic Decays of Vector Mesons

KeV	1967 T.D.Lee, Kroll Zumino	1967 DESY	1986
$\Gamma(\rho \to e^+e^-)$	———	7.0 ± 1.7	6.9 ± 0.3
$\Gamma(\omega \to e^+e^-)$	0.6	1.0 ± 0.3	0.66 ± 0.04
$\Gamma(\varphi \to e^+e^-)$	1.2	1.2 ± 0.4	1.31 ± 0.06

3. C-VIOLATION AND NEW PARTICLES

In my second year in Columbia, there were great excitements on the result by the Cronin and Fitch group on the long-lived K mesons decay to 2π mesons [ref. 9]. Among the many explanations offered at that time, the most stimulating one is again the one made by Professor Lee [ref. 10] which I recall as follows :

A scenario for CP violation : C-violation by the electromagnetic interactions.

The strong interactions are observed to conserve C to 1% :

$$\bar{P} + P \rightarrow \pi^+ + \pi^- + \ldots$$
$$\bar{P} + P \rightarrow K^+ + K^- + \ldots$$

The energy distributions of $\pi^+\pi^-$ (K^+K^-) are equal to 1%. On the other hand, there is no direct test of C conservation of the electromagnetic interactions. The rate of CP-violating decay $K_L \rightarrow \pi^+\pi^-$ relative to the CP-conserving mode is

$$\frac{K_L \rightarrow \pi^+\pi^-}{K_S \rightarrow \pi^+\pi^-} \approx 0.203$$

which is roughly α/π.

Thus it is of interest to assume that electromagnetic interactions violate C (particle-antiparticle conjugation), and follow through the consequences. For this purpose one must distinguish between :

C = Particle-antiparticle conjugation

C_γ = Charge conjugation

Assume the charge operator Q generally has an "ordinary" Q_j term and an"unusual" term Q_k :

$$Q = Q_j + Q_k$$

Under C	Under C_γ
$CQ_jC^{-1} = -Q_j$	$C_\gamma Q_j C_\gamma^{-1} = -Q_j$
$CQ_kC^{-1} = +Q_k$	$C_\gamma Q_k C_\gamma^{-1} = -Q_k$

The total charge Q always behaves in an ordinary way under charge conjugation C_γ. But due to the presence of the unusual charge Q_k, it behaves in an unconventional manner under particle-antiparticle conjugation C.

Experiments indicate that all known particles must have only normal type of charge (i.e. $Q_k = 0$). To have the unusual behavior, T.D. Lee postulates a new particle whose charge is entirely "unusual". The virtual effect of such a particle would then lead to C-violation in electromagnetic interactions.
This particle, called a, (dubbed the "leeon" by Leon Lederman), has the properties :

$$Q_k|a> = e|a>$$
$$C|a> = |a>$$

It is supposed to be stable under the strong and electromagnetic interactions, and decays through weak interactions.
Other properties, such as spin and mass, are open to speculations.

The novel idea is that each interaction may define their own symmetry operations C,P,T, with the same CPT. Observed symmetry violations are due to the mismatch between these definitions. For example :

Strong Interactions	**Electromagnetic**
$C_{st} = C$	$C_{em} = C_\gamma$
$P_{st} = P$	$P_{em} = P$
$T_{st} = C^{-1}C_\gamma T$	$T_{em} = T$

Experiments :

Search for a particles has started a new field of experiments on

$$P + P(N) \rightarrow \ell^+ + \ell^- + \ldots$$

First at B.N.L., then F.N.A.L. and ISR-CERN leading to many discoveries.

In the paper "Experimental Observation of a Heavy Particle J" [ref. 11], announcing the discovery of the J-particle in December 1974, it was stated clearly that "this experiment is part of a large program to study the behavior of timelike photons in p + p --> $\ell^+ + \ell^- + x$ reactions and to search for new particles which decay into e^+e^- and $\mu^+\mu^-$ pairs. The most striking feature of J is the possibility that it may be one of the theoretically suggested charmed particles or a's or Z_0's ".

A similar experiment later on, at Fermi Laboratory by Lederman's group led to the discovery of the famous Y particle [ref. 12].

The study of lepton pair production from hadron interactions is one of the best ways to search for new particles. It is one of the main research topics on particle physics today.

4. QUANTUM ELECTRODYNAMICS AND NEW PARTICLES

In 1967, I left Columbia and became a professor at M.I.T. In 1970, during one of my visits to Columbia, Lee explained to me his new theory on quantum electrodynamics [ref. 13], which I recall as follows :

A consistent finite theory of QED can be constructed by assuming that the vector potential has an imaginary part :

$$\varphi_\mu = A_\mu + iB_\mu$$

A_μ is the usual photon field.
B_μ is a new vector meson field that has negative metric.
Assume B_μ has mass m_B, the consequences are :

The photon propagator is modified to be :

$$D = \frac{i}{k^2}\left[\frac{m_B^2}{k^2 + m_B^2}\right]$$

with the corresponding corrections to the experiments :

$\left(\frac{d\sigma}{d\Omega}\right)_{e^+e^- \to \mu^+\mu^-}$ by a factor $\frac{m_B^2}{(k^2 + m_B^2)^2}$ and

the (g - 2) for μ meson by :

$$-\frac{\alpha}{3\pi}\left(\frac{m_\mu}{m_B}\right)^2$$

Experiments :
The search for the B particle may be the main reason why Professor Lederman and his group did the famous original experiment on "observation of massive muon pairs in Hadron collisions" [ref. 14]. In this experiment, they studied the reaction of

$$p + U \dashrightarrow \mu^+ + \mu^- + \text{anything}$$

reaching an incredibly small cross-section of 10^{-33} cm^2, and they interpreted their results in the following way :

"in an attempt to eliminate divergence difficulties in quantum electrodynamics, the possible existence of a "heavy photon" has been suggested. this object would modify the usual photon propagator by a multiplicative Breit-Wigner factor resulting in an integrated enhancement by a factor of about 100. The limit of ≥ 5 GeV/c^2 was set on the mass of the heavy photon".

We have continued the search for the B-particle at PETRA with the MARK-J experiment [ref. 15] by measuring very precisely the reaction

$$e^+ + e^- \dashrightarrow \mu^+ + \mu^-$$

We obtained :

$$\frac{h}{m_B c} < 10^{-16}$$

$$m_B > 200 \text{ GeV}/c^2$$

Continued searches will be carried out at LEP and at SLC accelerators.

5. NEW MATTER AND HEAVY ION PHYSICS

In recent years, one of the most important new areas of research has been experiments on heavy ion physics. This originated largely through the efforts of Professor Lee [ref. 16]. I have listened to many lectures on this subject by him, in Brookhaven, at CERN and at M.I.T. My understanding of Professor Lee's work in this field is as follows :

Background

Scalar fields (0^+ particles) have the special property that they have the quantum numbers of the vacuum.

In more familiar models, they are used to produce spontaneous symmetry breaking :
In the electro-weak theory, this is the mechanism for generating masses for the W, the Z, and for the quarks and leptons.

In the grand unified theories, they break the symmetry down to what we see at low energies, a process relevant in current theories of the early universe.

In these cases, the scalar fields develop vacuum expectation values. We can imagine that these expectations values can be changed in local regions, thus "exciting" the vacuum. In this sense the vacuum is a medium (like a dielectric, for example).

New Exploration

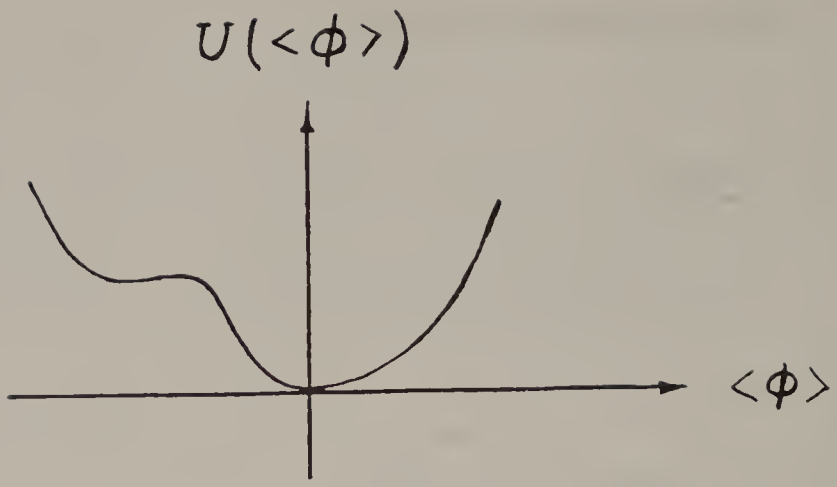
Suppose there is a 0^+ meson represented by the field φ.
Its vacuum expectation value can always be chosen as zero, by redefining φ through adding a constant, if necessary. Thus in the true vacuum

$$< \varphi > = 0.$$

But the field can be excited, so that in some portion of space $<\varphi> \neq 0$
The energy density depends on $<\varphi>$:

$$\text{Energy Density in Field} = U\ (<\varphi>)$$

which might look like the following drawing :



Now suppose this scalar field is strongly coupled to nucleons :

$$\text{Coupling} = g\bar{\Psi}\varphi\Psi$$

The mass term of the nucleon would be :

$$\bar{\Psi}(m + g\,\langle\varphi\rangle)\Psi,$$

where m stands for mass from other sources.

If we have a Fermi gas of nucleons in a region of space, the energy density would be the sum of that of the scalar field, and that of Fermi gas :

$$\text{Energy density} = U(\langle\varphi\rangle) + k\int_{pf} d^3p\sqrt{p^2 + (m+g\langle\varphi\rangle)^2}$$

where k is a constant to take care of such things like spin, and whether we have only neutrons (as in a neutron star), or protons and neutrons (as in a nucleus).

Because of the coupling to the scalar field, the effective mass of the nucleon is :

$$m_{eff} = |m + g\langle\varphi\rangle|.$$

In a "normal" vacuum we have $m_{eff} = m$. The integration over nucleon momentum p is restricted to the Fermi sphere, whose radius is the Fermi level p_F :

$$p_F = Cn^{1/3},$$

n = nucleon density,

C = constant.

Therefore the energy density depends on the nucleon density n.

Low Density : ($n \to 0$, hence $p_f \to 0$)
The field energy U ($\langle\varphi\rangle$) dominates. Minimizing, it leads to the "normal" vacuum with

$$\langle\varphi\rangle = 0$$

$$m_{eff} = m$$

High Density : ($n \to \infty$, $p_f \to \infty$)
The energy of the Fermi gas dominates. To minimize the energy, the single-nucleon energy wants to be as small as possible. So the effective mass becomes zero :

$$m_{eff} = m + g\langle\varphi\rangle = 0$$

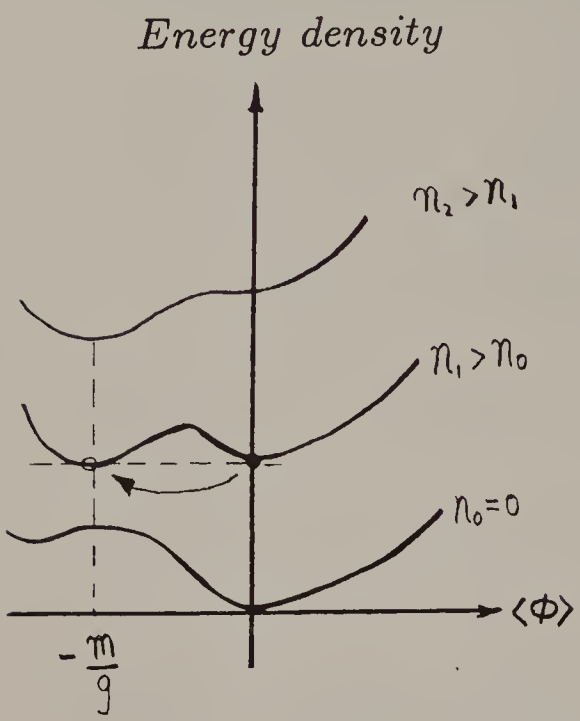
Thus the vacuum becomes "abnormal" with

$$\langle\varphi\rangle = -\frac{m}{g}$$

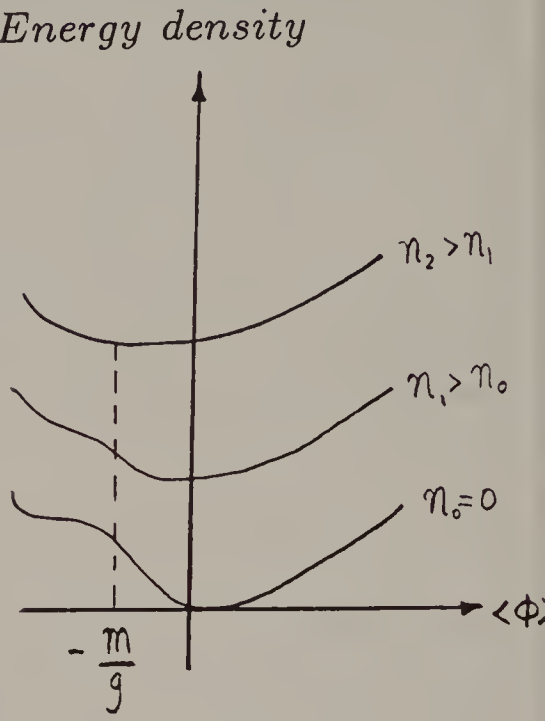
By increasing the nucleon density, we can induce a kind of "phase transition" of the vacuum in a region of space.

In the "abnormal" phase, the nucleon effective mass is zero.
As the nucleon density 'n' increases, the transition can be sudden or gradual, depending on the nature of the function U , as seen on the drawing enclosed hereafter :

Sudden Transition



Gradual Transition



We may attempt to see such a transition of the vacuum in heavy-ion collisions, in which nucleons are compressed to very high density in a short time.

This work has started a world-wide effort to study high energy neavy ion collision. There are now major experiments at Berkeley, at Brookhaven, and at CERN. With the possible construction of RHIC, a completely new field of exploration is foreseen to search for new form of matter.

6. SUMMARY

I have tried to list a few examples of the incredibly large amount of the original work by Professor Lee. From these few examples one can see that Professor Lee's work has played a fundamental important part on the experiments of :

(1) SEARCH FOR PROTON DECAYS,

(2) STUDY QED ; MEASURE THE SIZE OF LEPTONS,

(3) SEARCH FOR NEW PARTICLES and

(4) SEARCH FOR NEW FORM OF MATTER

THESE ARE THE MAIN TOPICS FOR EXPERIMENTAL PARTICLE PHYSICS TODAY.

With the construction of large accelerators, LEP, HGRA, SLC and RHIC, it is my hope that on Professor T.D. Lee's seventieth birthday, some of the questions raised by him will be answered.

I wish to thank Professor K. Huang, Professor G. Herten, Dr. P. Lecomte and Miss L. Barrin for their help in preparing this manuscript.

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I. I. Rabi

Caunterclackwise: S. Nagamiya, T. D. Lee, I. I. Rabi, T.-Y. Wu, E. Kalchin, G. C. Wick (partly hidden), V. L. Telegdi

INTRODUCTORY REMARKS

I. I. Rabi

Columbia University

As the first postwar chairman of the Physics Department and the oldest person around here, at least from the Columbia Physics Department, I want to welcome you all. I have this feeling, as Kennedy expressed himself at the time of his great dinner for the Nobel Prize recipients in Washington, "Never in the history of the world has there been such a concentration of brain power since Jefferson dined alone in the White House." And I can refer to another event: when Pauli and Bohr were here discussing Heisenberg's vaunted paper.

I want to say a few things. I first ran into T.D. Lee on my return from Mexico to Berkeley. Somebody told me there was a very bright postdoc. And he brought in T.D. Lee, a handsomer young physicist you never saw. And I was immediately impressed. This must have been about 1950 or 1951. It has been a great fortune to all of us that he came to Columbia and found this a congenial home. So this is a great celebration.

About ten years ago, my wife and I were at a Chinese restaurant downtown on Fifth Avenue celebrating our fiftieth anniversary, and in the same restaurant were the Yuans and the T.D. Lees, and they were celebrating T.D. Lee's fiftieth birthday. By the inexorable laws of arithmetic, this is also the sixtieth anniversary of my wedding to Helen. It is also the sixtieth anniversary of when I sent off my dissertation to the Physical Review. But more than that, overshadowing all of that, this is the sixtieth anniversary of the discovery or the invention--whatever you call it--of quantum mechanics, and that is a subject on which we all live and subsist. I do not know any name to ascribe to it. They were very, very great men. It is also the sixtieth anniversary of when I heard here at the physics colloquium the presentation of the results of Davis and Germer, establishing the dual nature of the electron. And ever since then, physics has been moving on at a tremendous rate, not only in breadth but also in depth. And there hasn't been a time when it hasn't been terribly exciting to be a physicist.

Thirty years ago I was on sabbatical leave at MIT when I got a call. Would I come to New York and preside at a press conference where they were to present the results of these terrific experiments of Wu and company, Lederman and Garwin? When I first heard about this, what the result was, I was completely flabbergasted. It was so unbelievable that an electron, a muon, could come out of disintegration and have itself correlated to the direction of motion. As a hands-on physicist, which I like to think of myself, how does it do it? You have to manufacture this thing and fix it up to the direction of motion. The whole thing was simply fantastic and I wouldn't have believed it at all if the results hadn't been so terribly remarkable. There was no question; these were 100% effects. It was really stupifying. So when I was asked to preside at the press conference, I said to myself you've got to make a description of this which is really true, but really understandable to the press and everyone else. Apparently so great was my authority at that time that they produced such a document. Some of you may remember it. When it was published in the New York Times and all over the world, it was an honest-to-goodness publication. It has been my principle: insist on the best and you'll get it. It was really, I think, a remarkable document at that time. And I haven't seen anything as simple since.

When I look at the faces and the people who are here, the physics I've lived through and the physicists I've known who are no longer with us, it really gives one hope that the generations renew themselves. In 1927 I went to Europe as a postdoc, and there were these great men--some of them younger than I was then--like Heisenberg and Pauli. Even Bohr was not so old at that time. It was such a great experience to live as a postdoc and as a graduate student at the birth of modern physics and to see quantum mechanics come out, at first as a trick of Heisenberg's and then as a trick by Schroedinger. We were a set of graduate students here at Columbia at that time. Kronig said to me,"Let us do something with Schroedinger's equation." So we looked in a book to see what hadn't been done and it was the symmetrical top. All right. So we set up the equations according to the formula given in Schroedinger's first paper. And we did the symmetrical top. Neither of us was very mathematical. But it was the first paper on the symmetrical top. So it is very important, you can tell your children and grandchildren, to be born at the right time. You come to grow up just when quantum mechanics is being made and you are in there both at the excitement and the surprises of it.

As I said, I was young; in fact, I was slightly older than the founders of quantum mechanics. They're all gone, I think, except deBroglie. But then, new generations do come along and we're still trying to make a science, an understandable science, a science that speaks to us as humans. I don't mean sentiment. I mean that as humans one goes around and gets certain fundamental experience--not knocking into trees, trying to learn directions of the

compass, trying to make understandable in a living way. We seem to get further and further from it, but we get nearer and nearer. The Copernican theory solved a lot of problems, and simplified a lot of problems. Certainly, quantum mechanics plus Pauli simplified so much of the structure of matter. We now have people coming along--some of them are here, some are too young to attend this--that will again make physics very simple and immediate in a human kind of way.

This is a long way from introducing Sidney D. Drell, and I think he was wondering when I'd come to the introduction. I was afraid he would call out and say "I want equal time." I won't say very much about him. In the first place, everybody knows Sidney Drell. Although he is a comparative youngster, compared to me, he is president of the American Physical Society. To be president of the American Physical Society is not what it used to be. At that time you were forced into it. When my term as president of the Physical Society was over, it was the custom that the retiring president was chairman of the nominating committee; and we nominated Enrico Fermi. So I passed on this notice to the secretary, Karl Darrow, and I got back a disturbing note saying that Professor Fermi wrote that he was very conscious of the honor and very pleased, but right now he was very busy and would be able to serve at some other time. As chairman of the nominating committee, I was in a dilemma. So I wrote back and said "I'm very distressed, Professor Fermi. Of course, I could call the committee together and nominate somebody else. But they come from all over the country and that seems to be impractical. We could have a conference call, but that doesn't sound intimate enough. Of course, I could write to each one of them and tell them of this problem, but I have no secretary." So I suggested to Professor Fermi that he could write to every member of the Physical Society asking them not to vote for him. Or, of course, he could just let it go and refuse to serve when the time came. Well, presented in that way, he gave up. And he was the president of the American Physical Society and made that famous speech, as retiring president, of the cyclotron whose circumference was the circumference of the earth. Unfortunately he died shortly after that, and it made me feel very good that I was that rude that I didn't accept his declination and that we could boast of having had Enrico Fermi as president of our society. I also added in my letter that when he consented to be a fellow of the Society, and in view of his great scientific reputation, he should have been aware of the dangers of accepting the fellowship and therefore being nominated for president.

Now Sidney D. Drell doesn't have this problem. Things have changed and you actually run for the office. You don't run away and have them catch you. The big difference is, at that time the candidate was not only scientifically eminent, but that is all he had to be. But now, not only does he have to be scientifically eminent, but to be a great guy. He has to have the interests of physicists at

heart; he has to give a program and take account of the political climate of the day, not in the Physical Society but in general. In other words, has to be a much more rounded person of great stature, and therefore Sidney Drell. Sidney.

S. D. Drell and T. D. Lee

THE MANY DIMENSIONS OF T. D. LEE

SIDNEY D. DRELL*

Stanford Linear Accelerator Center
Stanford University

Approximately thirty years ago, at about the time of the downfall of parity, or should I say its elevation to the more fundamental but only approximate symmetry of CP, the great American novelist and playwright Thorton Wilder turned sixty. On that occasion Supreme Court Justice Felix Frankfurter sent him a wire with the following message: "Welcome to the great decades". Today I also say:

> "Welcome T.D. to the great decades! Having beaten you there by two months, I can report that—at least so far—it's o.k. And there are some special advantages to look forward to. In a few years we can go to the movies at a cut rate and ride the buses and subways at half fare."

There are two very special pleasures of being a physicist: first of all, we enjoy sailing on one of the great adventure voyages of the human mind as we seek to discover what we are made of and what holds us together. And, secondly, we sail on this great adventure in the company of such wonderful friends, such marvelous people as the community here today; and it is a special pleasure to count Tsung Dao and Jeannette Lee as one's close friends.

Occasions like today have elements of fun as well as of seriousness. They provide great opportunities and wonderful excuses for reminiscing—which I will now proceed to do.

For scientific reminiscing, let me remind you of what the world was made of—or so we thought when T.D. and others of us were graduate students forty years ago. The basic nuclear glue was known. Sometimes we called it mesons and sometimes mesotrons. And I recall sitting in the University of Illinois Union for afternoon coffee with my two professors, Sid Dancoff and Arnold Nordsieck, the day the news arrived that the nuclear and cosmic ray mesons, the π and μ, were different—which led Nordsieck to suggest that we view this new development with great caution because one of those two presumed elementary particles would surely go away in short order—otherwise things would be too complicated!

Our understanding of elementary particles and processes has advanced enormously since then by any measure, but we have yet to match the masterful description of the world as presented early in the 1950's by Amos and Andy. I was reminded of their theory several weeks ago by a retrospective on their once-popular radio and television series that was shown on public television very late one evening out in California. Back in those times—when T.D. was studying

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viscosity, turbulence, statistical theories of equations of state and phase transitions, and the motion of slow electrons in polar crystals—Kingfish was explaining to Amos that the world was really made of protons, neutrons, fig newtons, and morons!

That was back around 1951—of course, long before one had learned about fig newtinos, smorons, and all those other Zuminos!

And there are many reminiscences of other happy occasions with T.D., away from physics. I don't remember when I first met T.D. but I do remember clearly, and I still savor the memory of, our first visit to Paris in the summer of 1958, following the Rochester conference—which that year, for the first time left Rochester to be held at that upstart new laboratory in Geneva. T.D. and I drove together from CERN to Paris to be greeted by a fantastic tour guide for our first visit to that lovely city. Our guide was David Pines, who had spent the year there and was already an accomplished bon vivant and a discriminating gourmet. Under David's tutelage we set a record on our first day in Paris that I believe still merits recognition in the Guinness Book of Records. It is theoretically possible to break our record, but I do not believe that in fact—on the basis of actual experiment—the record is vulnerable. On that day, T.D., David, and I ate a Michelin two-star lunch and a three-star dinner! After that we didn't have to—nor did we want to—eat again for the rest of the week! [Note added in proof: David Pines reports that on a subsequent occasion he and his wife Suzy did partake of two three-star meals on the same day! So much for that record.]

T.D. himself has had, as we well know, an enormous impact on the addiction of the American physics community to gourmet Chinese cuisine.

T.D. also had something to do about another gourmet occasion, which I regret to say never came to pass. Not everyone was immediately persuaded that the bold—or wild—suggestion of parity violation offered a sensible resolution to the $\tau - \theta$ puzzle. It would be false to suggest that I was readily enchanted by that proposal when I first heard it, but I remember arriving at Stanford in the summer of 1956 and trying to explain it to Felix Bloch. I worked hard at it—and gave it my best shot. Physics discussions with Felix were never easy or casual. At the end of our session Felix countered that he would eat his hat if this idea were confirmed by experiment. Felix did have a hat, which he rarely wore. I frequently reminded him of his obligation and offered a variety of appropriate seasonings. But long after the beautiful experiments that you heard about this morning had convinced him that parity, as he had known it, was violated, Felix failed to pay up on his bet.

As one looks over the broad landscape of physics it is truly very hard to find a region or a territory that has not been touched by T.D.'s incisive and enduring contributions, by his technical strength and virtuosity, or by his physical insights.

But let me remind you that the many dimensions of T.D.'s contributions as a teacher and statesman of physics are almost as impressive as his research achievements. First, as all of us who have enjoyed and learned from his lectures and seminars know, he excels in the art of classroom teaching, in giving

theoretical seminars, conference talks, and lecture series at summer institutes. He has been a superb teacher of us all. Additionally, T.D. has been more than the inspiration—he has been a personal teacher of an entire generation of young Chinese scientists as he has provided exceptional leadership and energy in order to help China recover from the damage to their science of the Cultural Revolution. In 1979 he lectured to some 1200 young Chinese physicists who assembled in Beijing to hear him hold forth on all modern physics—particle physics and statistical mechanics. This led to his classic book—PARTICLE PHYSICS AND INTRODUCTION TO QUANTUM FIELD THEORY—published in 1981. Therein you can find all of modern particle physics, circa 1981, assembled in 850 or so clear and concisely written pages, including symmetry theory, QCD and gauge theory, quark confinement, chiral theory, quark models and high energy processes, weak-electromagnetic unification and the like. The only defect in this volume is that it is written in the unforgivable East Coast metric of x, y, z and ict. Maybe at the start of his seventh decade T.D. will now outgrow that kid stuff of i's and convert to the superior West Coast metric. T.D. and I were both raised with x, y, z and ict notation in Wentzel's book on quantum field theory, but I do think it's time for T.D. to learn better. He should set that as one of his goals for the great decades ahead. By the way T.D., we're also waiting for you to write up for publication in English the other half of your 1979 lectures in China on statistical mechanics!

T. D., the teacher and educator, has also made it possible for more than 700 of China's brightest young physicists to come to our universities for their graduate education under what he calls the CUSPEA program (whatever that stands for), but we know them as Lee scholars. This is a program to give these scholars a superb modern training in physics while the education system in China is still rebuilding, so that upon returning home they can themselves be the professors for their succeeding generation of students. Does anyone doubt the tremendous importance of that investment in developing the latent talent and skills of such a great pool of brainpower for the future of man's culture and of the civilization of our planet, as well as for our understanding of nature? Without a doubt, I rate that program, dollar for dollar, as one of the most valuable investments a nation can make in its future.

And there is yet a fourth dimension to T.D.'s teaching activities. He has been the personal tutor of China's maximal leaders in helping them lead China to take great strides back to the scientific frontiers. Let me describe to you T.D.'s simple, sensible, and successful explanation to Deng Xiaoping of the need for postdoctoral research support and fellowships for the Lee scholars when they return home to China. As he described it to Deng, when teaching students in college, professors must pose the problems to them and also provide the correct answers. When students advance to the stage of writing a Ph.D. thesis, it is the task of the professor to pose the question and provide the problem—but not to know the answer ahead of time. That is to be determined by student's own research. What is important for the postdoctoral training is for young scientists to develop the capacity to pose problems on their own, as well as find their own

answers. Deng Xiaoping apparently understood that explanation very well, and China has now started a program to make it possible for their young postdoctoral fellows to develop their own new research programs in physics.

But let me go back a decade earlier and describe a 1974 meeting of Tsung Dao with then Chairman Mao Zedong which marked a very early important step in the commitment of the Chinese government to the education of bright young scientists.

In the prologue to the book being published this year by the University of Washington Press containing his Danz Lectures, T. D. describes how he answered Chairman Mao's question and explained to him why the concept of symmetry was so important. In Chinese, as described by T. D., the word "symmetry" carries the meaning of a static concept—*i.e.*, "the beauty of form arising from balanced proportions". But in Mao's view, as recounted by Tsung Dao, the entire evolution of human societies is based on dynamic change. Dynamics is the only important element, not statics. Mao felt strongly that it also had to be true in nature. Therefore, he was quite puzzled that symmetry should be elevated to such an exalted place in physics.

To illustrate the deeper dynamical meaning of symmetry to Mao, T.D. describes how as he sat talking to him in his residence inside the Imperial Palace. T.D. put a pencil on a pad of paper resting on the end table that was placed between their two chairs. Let me quote T.D.'s description of his demonstration:

> "I put a pencil on the pad and tipped the pad towards Mao and then back towards me. The pencil rolled one way and then the other. I pointed out that at no instant was the motion static, yet as a whole the dynamic process had a symmetry. The concept is by no means static; it is far more general than its common meaning, and applicable to all natural phenomena from the creation of our universe to every microscopic subnuclear reaction."

T.D. recounts that Mao appreciated the simple demonstration and then asked more questions about the deep meaning of symmetry and also about other physics topics, expressing regret that he had not had the time to study science. In the end of the conversation Mao accepted T.D.'s proposal that the education of at least the very brilliant young students should be maintained, continued, and strengthened. This led, with the strong support of Zhou Enlai, to the elite "youth class", a special intensive education program for talented students from the early teens through college. It was established first at the University of Science and Technology in Anhui and later, because of its success, also at other Chinese universities.

Let me turn next to the many dimensions of T.D.'s research achievements. The numerous major honors T.D. has received are evidence enough of his enormous contributions. His mark can be found in just about every area of physics. In statistical mechanics we look to the work of T.D., Yang, and collaborators for understanding the nature of phase changes, the theory of cluster expansions and the low-temperature behavior of hard-sphere boson systems, and for the analysis

of the general many-body problem in quantum statistical mechanics. In the area of weak interactions, one need only mention parity violation and all its ramifications, including high energy neutrino processes, experimental tests of CP, or T, invariance as well as various aspects of the heavy intermediate vector bosons. Most theorists have used the Lee model as a sandbox for some idea or other. T.D. has also made elegant studies of coherent states, or solitons, exploring bag models and abnormal vacuum states and their implications for high energy heavy-ion collisions. He has undertaken a major research program in lattice field theory, including the effects of gravity; and, in particular, he has developed a random lattice theory designed with a very elegant mathematical formalism to restore rotational symmetry properties to the lattice.

The day before I left Stanford last week to come East I received a package of four new papers by T.D. and collaborators which describe new possibilities for configurations of cold stellar matter based on soliton solutions in general relativity. They found coherent quantum states with masses up to as much as 10^{15} solar masses.

There isn't enough time today to describe all of T.D.'s contributions to physics—even if I could do it. So instead I took it as a personal challenge in preparing this talk today to find a topic in physics that T.D. has not solved or contributed to in his more than 200 published research papers—and to teach it to him. I have found such a topic and have been working on it in recent months with Dick Blankenbecler. This is the progress we have to report.

An important parameter in the design of very high energy electron colliders is the fractional energy loss due to bremsstrahlung as one beam pulse passes through the other pulse. This is known as beamstrahlung and has been treated by Himel and Siegrist by adapting a quantum treatment of synchrotron radiation by an electron in a uniform magnetic field given by Sokolov and Ternov. This adaptation necessarily involves several assumptions, in particular the approximation of the effects of the pulse by a uniform magnetic field in which the electron is orbiting as it radiates. In fact, the electron sees the rapidly approaching pulse in the collider frame of reference as transverse, mutually orthogonal electric and magnetic fields of equal strengths whose spatial dependence is determined by the distribution of charges in the pulse.

The challenge undertaken by Blankenbecler and me is to compute the energy loss more simply, more generally, and more accurately by a straightforward application of high energy scattering theory to the problem of radiation in the presence of a very strong field. In this case the field is the actual electromagnetic field of the pulse of positrons being traversed by a very energetic incident electron beam pulse.

Practical interest in this problem arises from the scaling laws governing the extension of electron-positron colliders from today's energy ceiling of 100 GeV at SLC and 150 GeV at LEP to up to the multi–TeV range. For linear colliders the increase of cost is roughly proportional to the increase of energy. In contrast, for

storage rings of radius R, the cost is given by the sum of two terms

$$\$ \propto (\text{Energy})^4/\text{R} + (\text{constant})\text{R} \tag{1}$$

where the first term measures the cost of the radiofrequency power required to replace the power radiated per turn by the circulating e^+e^- beams, and the second term represents the real estate cost of the ring.

The minimum of Eq. (1) occurs for radius R$\propto$ (Energy)^2 and shows that

$$\$ \propto (\text{Energy})^2 \text{ for storage rings}$$

whereas

$$\$ \propto (\text{Energy})^1 \text{ for linear colliders.}$$

The event rate for colliding beams is equal to the product of the interaction cross section and the luminosity which is defined by

$$\mathcal{L} = \frac{N_+ N_-}{\pi B^2}\text{f} \tag{2}$$

where $N_+(N_-)$ is the number of positrons (electrons) in the two colliding pulses, B is the common radius of the pulses and f is the frequency with which pulses collide. In practice, f is much smaller for liner colliders ($\sim$ 100 pulses/sec) than for storage rings in which f depends on the radius R and the number of pulses circulating in the storage ring, and is typically $\sim 10^5$/second. Therefore, by (2), in order to maintain a comparable event rate, the total charge Ne in each pulse must be higher and the radius B must be smaller for linear colliders. This means that the electromagnetic fields of the pulses will be very strong and cannot be treated perturbatively. Design numbers for the SLC and notional parameters for a "super" linear collider are listed below.

TABLE I

	SLC	"Super"
Beam energy (γm)	50 GeV; $\gamma = 10^5$	5 TeV; $\gamma = 10^7$
Pulse radius (B)	10^{-4} cm	5×10^{-8} cm
Pulse length (ℓ_o)	10^{-1} cm	3×10^{-5} cm
Number of charges (N)	5×10^{10}	3×10^8
Pulse frequency (f)	120/second	100/second
Luminosity ($\mathcal{L}$)	10^{31}/cm^2 -sec	10^{33}/cm^2 -sec
$\mathcal{L}\sigma_{point\ QED}$	0.3 events/hour	0.1 events/day

First consider the application of classical physics to study beamstrahlung. For simplicity, we choose to work in the rest system of one pulse (say the positrons) and consider radiation from one incident electron as it traverses the pulse at impact parameter b. In this reference frame, the pulse length is (Lorentz) stretched to $L = \ell_0\gamma$ and the incident electron energy is $p = 2\gamma^2 m$. Assuming the pulse is a uniform cylinder of radius B and length $L \gg B$ the transverse electric field acting on the incident electron is

$$e\vec{E}_\perp = \frac{-2N\alpha\vec{b}}{LB^2} \tag{3}$$

and the classical equation of motion is given by

$$\ddot{\vec{b}} = \frac{1}{p}\, e\vec{E}_\perp \tag{4}$$

In this calculation we treat the pulse as a fixed charge distribution which produces a static transverse electric field in its rest system. Therefore, we must limit our calculation to small changes in the electron's impact parameter b—*i.e.*, $\delta b \ll b$. This is known as small disruption of the beam. Otherwise, as the incident pulse of electrons is squeezed by the attractive field of the positron pulse, the radius of the positron pulse is likewise squeezed by the effect of the electron pulse. A proper treatment of these mutual focussing effects (which if large would set up betatron oscillations) would require a much more extensive and difficult analysis.

According to the classical equation of motion, the condition for small disruption can be expressed as

$$\frac{\delta b}{b} = \frac{1}{2pb}\,(eE_\perp)\,L^2 \approx \frac{y\ell_0}{2\gamma B} \ll 1 \tag{5}$$

where $y \equiv N\alpha/(mB)$ is a useful classical variable. Referring to the nominal 'super' parameters, we see that this restriction is satisfied since

$$\left.\frac{\delta b}{b}\right|_{super} \approx 6 \times 10^{-2}.$$

The approximation is not quantitatively valid for the SLC for which

$$\left.\frac{\delta b}{b}\right|_{SLC} \approx 0.4$$

and a more careful treatment is called for.

We can now calculate the fractional energy loss defined as the power radiated multiplied by the time required for the electron to traverse the pulse, divided by

the incident energy:

$$\delta(b)_{classical} = \frac{2}{3}\, e^2\, (2\gamma^2)^4\, |\ddot{b}|^2 \cdot L \cdot \frac{1}{p}$$

$$= \frac{16}{3}\, \frac{\alpha^3\, N^2\, \gamma}{m^3\, \ell_0\, B^2}\, \left(\frac{b}{B}\right)^2$$

Averaging over the impact parameter gives

$$\delta_{classical} = \frac{8}{3}\, \frac{\alpha^3\, N^2\, \gamma}{m^3\, \ell_0\, B^2} \tag{6}$$

This classical result which excludes all effects of radiation back on the motion of the radiating electron is valid only for values of $\delta_{classical} \ll 1$. Although this is valid for the SLC with $\delta_{classical}(SLC) \lesssim 10^{-2}$, it is grossly in error for the notional "super" for which $\delta_{classical}(super) > 10^5$!

In applying high energy quantum mechanical scattering theory to this calculation we identify three length scales that are important in characterizing the electron's path and radiation pattern:

1. the coherence length of radiation, ℓ_{coh}, defined as the path length of the electron corresponding to its acquiring a transverse momentum $\sim$ m from the electric field. Since the width of the photon radiation pattern is also $\sim$ m, the radiation can be coherent only from a finite length of the path, namely

$$\ell_{coh} \sim \left|\frac{m}{eE_\perp}\right| \sim \frac{L}{2y} = \frac{\ell_0}{2y}\gamma \tag{7}$$

2. the radiation length, ℓ_{rad}, related by the uncertainty principle to the reciprocal of the longitudinal momentum transfer,

$$\ell_{rad} \sim \frac{1}{q_z} \sim \frac{p}{m^2} \tag{8}$$

   where the last relation corresponds to giving a transverse momentum $\sim m$ to the target pulse.

3. the graininess of the bunch—*i.e.*, the average separation of its particles expressed by

$$\ell_{grn} \sim \frac{L}{N}. \tag{9}$$

In all cases of interest, the radiation length ℓ_{rad} is much larger than the graininess; i.e.

$$\ell_{rad} \sim \frac{p}{m^2} = \frac{2\gamma^2}{m} \gg \frac{L}{N} = \ell_{grn}. \tag{10}$$

This justifies our making a smooth approximation for the charge distribution of the pulse, as we did in (3). This applies for both the SLC–like parameters

corresponding to a dense pulse,

$$\frac{L}{N} \sim 2 \times 10^{-7} cm \ll B \sim 10^{-4} cm$$

and to those parameters quoted earlier as envisaged for a 'super' linear collider, corresponding to a dilute pulse,

$$\frac{L}{N} \sim 3 \times 10^{-6} cm \gg B \sim 5 \times 10^{-8} cm.$$

If the dimensionless ratio of the coherence length to the radiation length:

$$\frac{\ell_{coh}}{\ell_{rad}} \sim \frac{m\ell_0}{4\gamma y} \equiv C \tag{11}$$

is large, we are in the classical region, as appropriate for the SLC. In dimensional units $C \gg 1$ corresponds to the limit $\hbar \to 0$. In this regime the deflection of the electron orbit is negligible over a path length ℓ_{rad} and the form factor for radiation along the length ℓ_{rad} is unity. The result given in (6) can then be understood as radiation from L/ℓ_{rad} transverse slices of the pulse, each of thickness ℓ_{rad} and containing $N\ell_{rad}/L$ charges, with each slice radiating incoherently with respect to the others. Using (8), and introducing $d\sigma \propto \alpha^3 N^2 dk/(m^2 k)$ as the cross section for emission of a photon k by charge $N\alpha$, we find

$$\delta_{classical} \sim \frac{L}{\ell_{rad}} \frac{1}{\pi B^2} \int^p \frac{dk}{k} \left(\frac{k}{p}\right) \left(\frac{N\ell_{rad}}{L}\right)^2 \frac{\alpha^3}{m^2} \sim \frac{N^2 \alpha^3 \gamma}{m^3 \ell_0 B^2} .$$

In the quantum regime of small $C \ll 1$ as appropriate for the 'super', we calculate the form factor for the overlap of the radiation along the bending path ℓ_{coh} and find that it varies as $C^{4/3}$, clearly showing the diminishing overlap in this situation.

To formally carry out this calculation we must derive an expression for the matrix element for the emission of a photon during the scattering of an electron from a pulse of N positrons:

$$M = \left\langle \phi_f^{(-)} \middle| \vec{A} \cdot \vec{J} \middle| \phi_i^{(+)} \right\rangle , \tag{12}$$

where $\vec{A}$ is the photon field, $\vec{J}$ is the electron current and $\phi_f^{(-)}$ and $\phi_i^{(+)}$ are respectively the final (incoming) and initial (outgoing) scattering eigenstates of the electron in the static external field of the pulse.

Blankenbecler and I have now completed this analysis* (at the time of the talk it had not yet been completed) and I will just summarize the highlights:

*SLAC–PUB–4186, January 1987.

1. The electron's scattering eigenstates must be constructed one order beyond the eikonal approximation in powers of 1/(energy). This corresponds to keeping terms of order 1/p in the phase of the wave function because they are of the same order of magnitude as the characteristic longitudinal momentum transfers as indicated in (8).
2. The wave function phase is of order $\sim N\alpha \gg 1$ so that some care is required in the formal analysis. We show that it is possible to do the matrix element integral over the impact parameter by the method of stationary phase and obtain the appropriate quantum corrections to the classical orbit.
3. After a series of manipulations we end up with remarkably simple scaling forms for the fractional energy loss and the power spectrum. In particular, the average energy loss obeys the scaling law $\delta = \delta_{classical} F(C)$ where $C = \frac{m\ell_0}{4\gamma y}$ as defined earlier and form factor F(C) has a simple limiting behavior

$$F(C) = 1 - \frac{4.77}{C} \qquad \text{for} \qquad C \gg 1$$

and

$$F(C) = 0.83C^{4/3}(1 - 2C^{2/3}) \qquad \text{for} \qquad C \ll 1$$

These results are useful for choosing parameters in the design of colliders with specified values of δ and of the radiated power spectrum. Our results quantitatively confirm the arguments of Himel and Siegrist and their adaptation of synchrotron radiation formulas to the collider in the extreme quantum limits $C \ll 1$.

Finally T.D., as a memento of this happy occasion, I want to present to you on behalf of all your Stanford friends and colleagues a SLAC beam tree inscribed in honor of your 60th birthday. It is an example of nature's beautifully artistic symmetry-breaking.

R. Friedberg and T. D. Lee

T. D. Lee and N. Cabibbo

G. Charpak and B. Zumino

G. C. Wick and J. Lee

INTRODUCTORY REMARKS

Gian Carlo Wick

Scuola Normale Superiore

Listening to the talks since 9 o'clock this morning, I have become more and more worried, realizing that the chairman must be or ought to be, if possible, a very well-known, experienced and skillful speaker and also have some very good jokes. The ideal thing is if you have both, as the preceding chairman did. Well, I do not qualify in this category. And I am going to talk very seriously. I will not try to add to what has been said already about all the things for which physics is indebted to T.D., which is of course the important question. But this has been done already so well that I don't know that I can add much. So I will speak about a less important question, and that is why I am indebted to T.D. It has some meaning because it also shows what a kind and, in a certain sense, what a forgiving person he is. You see, he was in Pisa some years ago and gave a talk in which he described me as a gentleman. And that was very generous because I played a dirty trick on him many years ago when he was very young. I really didn't mean to. But it happened. Jack-- Jack Steinberger--was there and I knew Jack. I had worked a short while with him in Chicago before leaving there and going to Berkeley. And Jack knew I was looking for a Research Associate. And he told me, "You ought to know there is a very young student--extremely young--in Chicago. But he's very bright, I think you want him." So I wrote to T.D. and he accepted. But what I didn't know-- and all my friends tell me I didn't know it because I was foolish--but I didn't know that Berkeley was in the process of becoming a real hornet's nest. And when T.D. arrived there, he obviously very soon discovered that fact. It didn't hit him in particular; he didn't lose his job. But I did. And that was annoying to him because you see he lost his sponsor the moment he arrived. I had learned that fifteen days earlier. So I didn't feel responsible except for the fact, as my friends tell me, that I should have known it much earlier. Well, he obviously didn't bear any grudge with me for that, because many, many years later he reciprocated by doing just the opposite thing. I think he had something to do with the fact that I was invited to join this department in the early '60's. And that was a

great service to me, not that I didn't like the place I was in. I was in Brookhaven at that time. But coming here meant that I had an opportunity to collaborate with T.D., and this happened also at a certain age, when most people tend to become formalists; instead of doing physics, they start doing mediocre mathematics. But T.D. kept me out of that because he kept asking me questions that had something to do with physics. And so in this way he made me stay a little younger for a while.

Well, I had another reason to be grateful for becoming a professor at this department. Namely, I have a sentimental attachment to this department in the sense that my--I don't know if it was the first paper I wrote or the second paper I wrote--but when I arrived in America I discovered that that paper was known to the people in Columbia, in particular to Professors Rabi and Ramsey, and they quoted that paper to me, which was very important to me because it was my first paper. They knew it because it had something to do with molecular beams and the properties of hydrogen molecules, and so on. They were probably the only people in the U.S. who knew about this paper. So this was very pleasant to me. Well, I could go on saying how pleasant it has been to work with T.D., in particular, just the fact that what he was doing was always physics. It reminds me about an episode about Fermi which is described by Segrè in the introduction to Fermi's collected papers. When Fermi was a student in high school, he had an adviser who was a friend of the family. And this adviser asked him, "Well, when you go to the university, what do you want to do? You probably should either do physics or mathematics." And Fermi looked at him and said, "Well, I am only interested in mathematics because it will help me to do physics." In other words, this was a very sharp knowledge of exactly what you're going to do. And I think T.D., in a certain sense, shares that sense of very well-defined direction. And that was good for me also.

Well, I should say something about Zumino. I also knew Zumino when he was a student, I think. He was a student in Rome, and he was very clearly a very bright student. I didn't see him for a long time after the War, and then I met him in the U.S. Since I have known him I consider him--well, he's not only a very good physicist--but he's one of the most lucid speakers that I have heard. When he describes something, there are no loose points. It's all extremely clear, at least if you are able to understand it. I have to say that because I suffer from a difficulty that I think J.J. Thompson described. He said that after you're past a certain age, and I won't mention that age because I don't want to offend anybody, you shouldn't judge modern physics anymore. What I am surprised at, is that Zumino is still doing modern physics. But he is quite a bit younger than I, so that's one explanation. I am sure you are going to enjoy his talk.

GEOMETRY AND PHYSICS*

Bruno Zumino

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1. Introduction

I would like to describe in this lecture some aspects of geometry which have become relevant to physics during the past few years. The importance of differential geometry, topology and of concepts such as that of connection and curvature on fiber bundles has been recognized for some time. These concepts are important, for instance, in the study of classical solutions of Yang–Mills theory and relativity. More recently, a knowledge of the theory of line bundles and $U(1)$ bundles has become an essential tool for the understanding of a number of subjects. Among them are the anomalies in quantum field theory and string theories and various properties of Riemann surfaces and their moduli spaces, which are relevant for string theories. In this lecture I shall try to outline, by means of a simple example, some basic facts about line bundles for the inexperienced physicist. The best known physical example would be the Dirac point monopole. Instead, I shall describe the quantum mechanical phenomenon called Berry's phase, where the same mathematics emerges, without reference to physical monopoles. No new results will be presented. My aim is to be as clear and pedagogic as possible, even at the cost of mathematical rigor. Only some familiarity with exterior differentiation is assumed. I shall be satisfied if I have aroused enough curiosity to motivate the reader to look into the appropriate literature. Among the mathematical references [1–5], Refs. [2] and [3] are written for physicists and are very readable. Actually, [1], [4] and [5] are also quite readable, if one is willing to make a little effort. I quote only a few physics papers on anomalies [7–9], on strings [10–13] and on Berry's phase [14–16], chosen because they are representative of the use of the mathematical methods mentioned above. Reference [6] is a collection of papers and lecture notes and contains numerous references to earlier literature. A rather mathematical introduction to geometric quantization is given in [17].

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This lecture is dedicated to T. D. Lee on the occasion of his 60th birthday. It is a small way to express my admiration for his qualities as a scientist and as a person and my gratitude for the friendship he has shown me on many occasions.

2. Non Integrable Quantum Phase

Let $H(\lambda)$ be a quantum mechanical Hamiltonian which depends upon a certain number of parameters $\lambda = (\lambda_1, \lambda_2, \cdots \lambda_k)$ and let $|\lambda>$ be the normalized eigenvector corresponding to a given discrete isolated nondegenerate eigenvalue $E(\lambda)$

$$H(\lambda)|\lambda>= E(\lambda)|\lambda> . \tag{1}$$

The eigenvalue will remain nondegenerate as λ varies in an open neighborhood in parameter space. The eigenvector $|\lambda>$ is determined only up to a phase, which can be different for different λ. We define the gauge potential

$$A = A_i(\lambda)d\lambda^i = i < \lambda|d|\lambda >= -i(d < \lambda|)|\lambda >, \tag{2}$$

where d denotes differentiation in λ, or more explicitly

$$A_i(\lambda) = i < \lambda|\frac{\partial}{\partial\lambda^i}|\lambda >= -i\left(\frac{\partial}{\partial\lambda^i} < \lambda|\right)|\lambda > . \tag{3}$$

A change of phase

$$|\lambda >\rightarrow |\lambda > e^{-i\beta(\lambda)} \tag{4}$$

induces a gauge transformation

$$A \rightarrow A + d\beta. \tag{5}$$

As defined, the gauge potential is real, since the eigevectors are normalized. The corresponding gauge invariant field strength is

$$F = dA = i(d < \lambda|)d|\lambda >, \tag{6}$$

or more explicitly

$$F_{ij} = i\left(\frac{\partial}{\partial\lambda^i} < \lambda|\right)\left(\frac{\partial}{\partial\lambda^j}|\lambda >\right) d\lambda^i \wedge d\lambda^j. \tag{7}$$

Remember that exterior differentials anticommute and that $d^2 = 0$. From now on we shall understand products of differentials to be exterior products and omit the wedge sign $\wedge$.

If the eigenvectors $|\lambda>$ are not normalized, (2) is replaced by

$$A = \frac{i}{2}\frac{<\lambda|d|\lambda> - (d<\lambda|)|\lambda>}{<\lambda|\lambda>} \tag{8}$$

and (6) becomes

$$F = dA = i\frac{1}{<\lambda|\lambda>}\left((d<\lambda|)d|\lambda> - \frac{(d<\lambda|)|\lambda><\lambda|d|\lambda>}{<\lambda|\lambda>}\right). \tag{9}$$

These expressions are sometimes more convenient for actual computations than (2) and (6). One can now consider more general gauge transformations

$$|\lambda> \rightarrow |\lambda>[\zeta(\lambda)]^{-1}, \tag{10}$$

$$A \rightarrow A - \frac{i}{2}d\ln\frac{\zeta}{\bar{\zeta}}, \tag{11}$$

where ζ may have modulus different from one. Here and in the following the bar denotes complex conjugation.

It is obvious, from (2) or (8), that, if the vectors $|\lambda>$ can be chosen to have real components in a finite neighborhood of parameter space, then A will vanish and consequently F will also vanish.

In quantum mechanics, a physical state is represented by a ray of non vanishing vectors differing by a complex proportionality factor. It is clear that the above potential cannot be defined on the space of rays, one needs the Hilbert space of vectors, or at least the space of normalized vectors. One can describe a ray by choosing a representative vector belonging to it, but this choice is obviously not unique. The arbitrariness gives rise to the gauge transformation (5) or (11), different choices of representative correspond to different gauges. On the other hand, the field strength F is gauge invariant. This means that it must be possible to define it on the space of rays itself.

Consider a vector $|>$ in the ray corresponding to a given eigenvalue $E(\lambda)$. It has the form

$$|> = \psi|\lambda>, \tag{12}$$

where ψ is a non vanishing complex number and $|\lambda>$ is a particular vector which we choose as representative for the ray. A different choice of representative

$$|\lambda>' = [\zeta(\lambda)]^{-1}|\lambda> \tag{13}$$

will determine the same ray. The same vector (12) will now be given by

$$|> = \psi'|\lambda>', \tag{14}$$

where

$$\psi' = \zeta(\lambda)\psi. \tag{15}$$

By letting the complex function $\zeta(\lambda)$ depend on λ we have explicitly allowed the possibility that, for different λ, we change the representative by a different factor. While λ labels the various rays, ψ is a coordinate which distinguishes different vectors in a particular ray. For given λ, (15) is a change of coordinates corresponding to the change of basis (13). The length of the vector (12) is given by

$$k(\psi, \bar{\psi}, \lambda) = < | > = |\psi|^2 \, h(\lambda), \tag{16}$$

where

$$h(\lambda) = < \lambda | \lambda > . \tag{17}$$

Under the transformation (13), h changes into

$$h'(\lambda) = |\zeta(\lambda)|^{-2} \, h(\lambda). \tag{18}$$

We define the one–form

$$\sigma = \frac{< |d| >}{< | >} = d \ln \psi + \frac{< \lambda | d | \lambda >}{< \lambda | \lambda >} \tag{19}$$

and the complex conjugate

$$\bar{\sigma} = \frac{(d < |) | >}{< | >} = d \ln \bar{\psi} + \frac{(d < \lambda |) | \lambda >}{< \lambda | \lambda >}. \tag{20}$$

They are invariant under the gauge transformation (13), (15) and so is the real combination

$$B = \frac{i}{2}(\sigma - \bar{\sigma}) = \frac{i}{2} d \ln \frac{\psi}{\bar{\psi}} + A. \tag{21}$$

As we shall see in Section 3, in mathematical language we are dealing here with a line bundle. The rays corresponding to physical states are complex lines in Hilbert space. The set of all vectors is the collection of all points of all lines, a line bundle. The set of all rays is the base of the bundle, each ray is as fiber of the bundle. The eigenvector corresponding to a given eigenvalue, as λ varies in parameter space, spans a sub–bundle of the total bundle of all vectors. The one–form σ is a connection form on this sub–bundle and its vanishing defines parallel transport, i.e. it associates to a vector in a given ray a vector in a ray infinitesimally close to it. If we limit ourselves to normalized vectors, the coordinate ψ in the ray must have absolute value one. The vectors in a given ray differ just by a phase, which can be identified with an element of the group $U(1)$. One speaks then of a $U(1)$ bundle, rather than a line bundle.

To illustrate the above, we consider the standard example of a 2×2 Hermitian matrix (two level system, spin $\frac{1}{2}$ in magnetic field)

$$H(\vec{\lambda}) = \vec{\lambda} \cdot \vec{\sigma} = \begin{pmatrix} \lambda_3 & \lambda_1 - i\lambda_2 \\ \lambda_1 + i\lambda_2 & -\lambda_3 \end{pmatrix}. \tag{22}$$

The eigenvalues are $\pm\lambda$, where $\lambda \equiv |\vec{\lambda}|$. Consider, for instance, the eigenvalue $+\lambda$. One can take as eigenvector

$$|\vec{\lambda}> = \begin{pmatrix} \lambda + \lambda_3 \\ \lambda_1 + i\lambda_2 \end{pmatrix}. \tag{23}$$

This eigenvector is not normalized

$$< \vec{\lambda}|\vec{\lambda} >= 2\lambda(\lambda + \lambda_3). \tag{24}$$

It is very easy to apply (8) and (9) with the result

$$A = -\frac{1}{2}\frac{\lambda_1 d\lambda_2 - \lambda_2 d\lambda_1}{\lambda(\lambda + \lambda_3)} \tag{25}$$

and

$$F = -\frac{1}{2}\frac{\lambda_1 d\lambda_2 d\lambda_3 + \lambda_2 d\lambda_3 d\lambda_1 + \lambda_3 d\lambda_1 d\lambda_2}{\lambda^3} \tag{26}$$

(remember that exterior differentials anticommute). In terms of the usual azymuthal angle ϕ of spherical coordinates

$$e^{i\phi} = \frac{\lambda_1 + i\lambda_2}{\sqrt{\lambda_1^2 + \lambda_2^2}} \tag{27}$$

and polar angle θ

$$\lambda_3 = \lambda \cos\theta, \tag{28}$$

the gauge potential is

$$A = -\sin^2\frac{\theta}{2}\, d\phi. \tag{29}$$

Equations (25) and (26) have the form of the gauge potential and the magnetic field of a Dirac point monopole of strength $-\frac{1}{2}$ situated at the origin of parameter space. The gauge potential is not defined globally, as made clear by the singularity (Dirac string) along the negative λ_3 axis. The normalized eigenvector is

$$\frac{1}{\sqrt{2\lambda(\lambda + \lambda_3)}} \begin{pmatrix} \lambda + \lambda_3 \\ \lambda_1 + i\lambda_2 \end{pmatrix} = \begin{pmatrix} \cos\frac{\theta}{2} \\ e^{i\phi}\sin\frac{\theta}{2} \end{pmatrix}. \tag{30}$$

The eigenvector (30) has a phase ambiguity on the negative λ_3 axis, $\theta = \pi$. If one multiplies (30) by the λ dependent phase $e^{-i\phi}$, one obtains the new normalized eigenvector

$$\frac{1}{\sqrt{2\lambda(\lambda - \lambda_3)}} \begin{pmatrix} \lambda_1 - i\lambda_2 \\ \lambda - \lambda_3 \end{pmatrix} = \begin{pmatrix} e^{-i\phi}\cos\frac{\theta}{2} \\ \sin\frac{\theta}{2} \end{pmatrix}. \tag{31}$$

Now the phase ambiguity is on the positive λ_3 axis, $\theta = 0$. The corresponding gauge potential has a singularity on the positive λ_3 axis and is given by

$$A + d\phi = \frac{1}{2} \frac{\lambda_1 d\lambda_2 - \lambda_2 d\lambda_1}{\lambda(\lambda - \lambda_3)}, \tag{32}$$

where, as one can easily see from (27),

$$d\phi = \frac{\lambda_1 d\lambda_2 - \lambda_2 d\lambda_1}{\lambda_1^2 + \lambda_2^2}. \tag{33}$$

The new potential is obtained from the old one by a singular gauge transformation. The gauge function ϕ is multivalued and undetermined along the entire λ_3 axis. The curvature F given by (26) is independent of gauge transformations.

The origin of the string–like singularity can be understood as follows. There is an exceptional point $\vec{\lambda} = 0$ in parameter space, where the two eigenvalues $\pm\lambda$ coincide. If we remove this point of degeneracy, parameter space acquires a non trivial topology. From the expressions (30) or (31) for the normalized eigenvector one can see that, once the phase is fixed, the eigenvector depends only on the direction of the vector $\vec{\lambda}$. Therefore we are really dealing with a $U(1)$ bundle having as base a sphere around the origin in $\vec{\lambda}$ space. The situation is exactly the same as for the Dirac monopole. Since the base manifold is topologically non trivial, the $U(1)$ bundle can also be non trivial.

If we move along a closed curve C in parameter space, starting say at the point $\vec{\lambda}_0$, a vector

$$|\vec{\lambda}_0 > e^{i\alpha_0} \tag{34}$$

of the fiber at λ_0 can be parallel transported in the $U(1)$ bundle and will come back as the vector

$$|\vec{\lambda}_0 > e^{i(\alpha_0 + \Delta\alpha)} \tag{35}$$

of the same fiber at λ_0, where

$$\Delta\alpha = \oint_C d\alpha = \oint_C A, \tag{36}$$

since $\psi = e^{i\alpha}$ and parallel transport means $B = 0$. By Stokes' theorem this can be tranformed into

$$\oint_C A = \int_S F, \tag{37}$$

where the surface S has the curve C as its boundary, $C = \partial S$. In the example described above, $\Delta\alpha$ equals the magnetic flux through the surface S of the magnetic field of a Dirac monopole of strength $-\frac{1}{2}$ at the origin. Equivalently, it equals $-\frac{1}{2}$ times the solid angle corresponding to the surface S as seen from the origin.

Observe that the surface S is not uniquely determined by the curve C. If S is deformed smoothly the solid angle remains the same until the surface goes through the origin when the solid angle changes by $\pm 4\pi$. Then $\Delta\alpha$ changes by $\mp 2\pi$ so that the phase $e^{i\Delta\alpha}$ is unaffected. In general, the vector obtained from a given vector by parallel transport along a closed curve is unique: it differs from the given vector by the (non integrable) phase factor $e^{i\Delta\alpha}$, called Berry's phase, which is of completely geometric nature. Berry [14] has shown that this geometric phase factor arises in a variety of examples in addition to the well known dynamic phase factor

$$e^{-\frac{i}{\hbar}\int_0^T E(\lambda(t))dt} \tag{38}$$

when a quantum mechanical system is transported adiabatically along a closed curve in parameter space. The mathematical interpretation in terms of a $U(1)$ bundle is due to Barry Simon [15].

In the next section we shall see some general definitions and properties of line bundles. The example of this section can provide a concrete reference.

3. Line Bundles and $U(1)$ bundles

Consider a complex manifold M. In an open neighborhood we can use complex coordinates $t_1, t_2, \cdots t_n$. A line bundle over M associates a complex line as fiber to each point of the base M. Let the complex number ψ be a coordinate for the fiber. A connection is a one–form ω whose vanishing defines parallel transport of the (one dimensional) complex vectors of the fiber. In local coordinates one can write

$$\omega = d\psi + \Theta\psi, \tag{39}$$

where Θ is a one–form on M. Let there be a metric for the line bundle, given in local coordinates by

$$k = |\psi|^2 h(t, \bar{t}), \tag{40}$$

where h is a real non vanishing function. A metric compatible connection satisfies

$$dk = 0 \tag{41}$$

when the vector in the fiber is parallel transported

$$d\psi + \Theta\psi = 0, \tag{42}$$

$$d\bar{\psi} + \bar{\Theta}\bar{\psi} = 0. \tag{43}$$

Since ψ is arbitrary, this gives

$$dh - (\Theta + \bar{\Theta})h = 0 \tag{44}$$

as the condition for metric compatibility. We require Θ to be of type (1,0), which means that it contains only the differentials $dt_1, \cdots dt_n$ and not $d\bar{t}_1, \cdots d\bar{t}_n$. Then the metric compatibility equation separates into the two equations

$$\partial h = \Theta h, \tag{45}$$

$$\bar{\partial} h = \bar{\Theta} h, \tag{46}$$

with solutions

$$\Theta = h^{-1}\partial h = \partial \ln h, \tag{47}$$

$$\bar{\Theta} = h^{-1}\bar{\partial} h = \bar{\partial} \ln h. \tag{48}$$

We recall here the well known definitions

$$d = \partial + \bar{\partial},$$

$$\partial = dt^i \frac{\partial}{\partial t^i}, \qquad \bar{\partial} = d\bar{t}^i \frac{\partial}{\partial \bar{t}^i} \tag{49}$$

and the relations

$$d^2 = \partial^2 = \bar{\partial}^2 = \partial\bar{\partial} + \bar{\partial}\partial = 0. \tag{50}$$

Observe that

$$\Theta + \bar{\Theta} = d \ln h. \tag{51}$$

The curvature two–form F can be defined from the equation

$$d\omega + \Theta\omega = -iF\psi, \tag{52}$$

where the left hand side is the covariant differential of ω. As defined, F is real and equals

$$F = id\Theta = -id\bar{\Theta} = dA, \tag{53}$$

where we have defined an equivalent real gauge potential

$$A = \frac{i}{2}(\Theta - \bar{\Theta}) = i\Theta - \frac{i}{2} d \ln h. \tag{54}$$

Using (53) we see that the curvature is given in terms of the metric by

$$F = i\bar{\partial}\partial \ln h = \frac{i}{h}\left(\bar{\partial}\partial h - \frac{\bar{\partial} h \partial h}{h}\right). \tag{55}$$

Clearly the connection form Θ is more convenient if one is interested in analiticity, A if one is interested in reality properties. A corresponds to the real gauge potential of Section 2.

By definition the curvature two–form satisfies the Bianchi identity

$$dF = 0, \tag{56}$$

i.e. it closed. Normalized by dividing it by -2π, the curvature is called the Chern form and its integral over a closed surface in M gives an integer, the Chern number.

A line bundle is a vector bundle, the fiber is a one–dimensional vector space, in our case complex. A change of basis in the vector space is a linear transformation, in our case it changes the coordinate ψ into

$$\psi' = \zeta\psi, \tag{57}$$

where $\zeta(t)$ is a non vanishing complex number, in general different for different fibers. Since the metric k must be invariant, h must change into

$$h' = |\zeta|^{-2}\, h. \tag{58}$$

Correspondingly Θ changes by a gauge transformation into

$$\Theta' = \partial \ln h' = \Theta - \partial \ln \zeta \tag{59}$$

and we have

$$\bar{\Theta}' = \bar{\Theta} - \partial \ln \bar{\zeta}, \tag{60}$$

$$A' = A - \frac{i}{2} d \ln \frac{\zeta}{\bar{\zeta}}, \tag{61}$$

$$\omega' = \zeta\omega, \quad \bar{\omega}' = \bar{\zeta}\bar{\omega}. \tag{62}$$

The null vector of the fiber remains null if we make the above change of coordinates. The null vector also remains null by parallel transport. The null vectors of all fibers form the null section, which is defined globally. For a nonvanishing vector ψ the metric k never vanishes. If ψ is nonzero we can define the one–form

$$\sigma = \frac{1}{\psi}\omega = d \ln \psi + \Theta = k^{-1}\partial k, \tag{63}$$

where differentiation operates now also on ψ. Under the transformation (57), (58), σ is invariant and so is the real combination

$$B = \frac{i}{2}(\sigma - \bar{\sigma}) = \frac{i}{2}(\partial - \bar{\partial}) \ln k. \tag{64}$$

The curvature (53) is also given by

$$F = dB = id\sigma = -id\bar{\sigma}. \tag{65}$$

The following example is relevant for Section 2 and illustrates also very clearly an important point.

Consider an $n+1$–dimensional Hilbert space whose vectors have complex components $z_i = z_0, z_1, \cdots z_n$. A physical state is described by a ray of non vanishing vectors differing from each other by a complex proportionality factor. The space of all rays is n–dimentional complex projective space $P_n(C)$. Since the vectors of a ray are non vanishing, for a given ray at least one of the coordinates will be different from zero, say z_j. By continuity z_j will be non vanishing for neighboring rays as well. The rays of $P_n(C)$ for which z_j does not vanish form an open neighborhood U_j of $P_n(C)$. In U_j one can use as local coordinates for $P_n(C)$ the n rations $z_0/z_j, z_1/z_j \cdots z_n/z_j$, omitting z_j/z_j which equals 1. The entire $P_n(C)$ is covered by the $n+1$ neighborhoods $U_0, U_1, \cdots U_n$. The system of coordinates which is valid in each neighborhood cannot be extended to the entire $P_n(C)$, but the space can be described by using local coordinates in each neighborhood and giving the transformation functions in the overlap of two neighborhoods. Alternatively, one could use $z_0, z_1, \cdots z_n$ as homogeneous coordinates.

There is a natural line bundle over $P_n(C)$. It has $P_n(C)$ as base and the fiber over a point of $P_n(C)$ consists of all vectors belonging to the ray associated to that point. It is convenient to take out the null vector of each ray, then the natural line bundle can be identified with the original Hilbert space minus the null vector. The hermitean metric of the Hilbert space

$$k = |z_0|^2 + |z_1|^2 + \cdots + |z_n|^2 \tag{66}$$

induces a natural metric on the line bundle. For instance in U_0, using local coordinates

$$t_1 = \frac{z_1}{z_0}, t_2 = \frac{z_2}{z_0}, \cdots t_n = \frac{z_n}{z_0}, \tag{67}$$

we have

$$k = |z_0|^2 (1 + |t_1|^2 + \cdots + |t_n|^2). \tag{68}$$

Comparing with (40), we can say that, in a special gauge, $z_0 = \psi$ is the coordinate for the fiber and

$$h = 1 + |t_1|^2 + \cdots + |t_n|^2 . \tag{69}$$

Equations (54) and (55) now give, for the real gauge potential,

$$\begin{aligned} A &= \frac{i}{2}(\partial - \bar{\partial}) \ln h \\ &= \frac{i}{2} \frac{\bar{t} \cdot dt - t \cdot d\bar{t}}{1 + \bar{t} \cdot t} \end{aligned} \tag{70}$$

and for the curvature form

$$F = dA = i\frac{1}{1+\bar{t}\cdot t}\left(d\bar{t}\cdot dt - \frac{d\bar{t}\cdot t\,\bar{t}\cdot dt}{1+\bar{t}\cdot t}\right). \tag{71}$$

Up to a factor this is the Kähler form associated with the well known hermitean Fubini–Study metric on $P_n(C)$($P_n(C)$ is a Kähler manifold) [2]. We have used the abbreviation

$$\bar{t}\cdot dt = \sum_{i=1}^{n} t_i dt_i \tag{72}$$

and so on.

In a different gauge h is replaced by

$$\begin{aligned} h' &= |\zeta(t)|^{-2}(1+\bar{t}\cdot t) \\ &= |u_0|^2 + |u_1|^2 + \cdots + |u_n|^2, \end{aligned} \tag{73}$$

where we have defined the t dependent vector

$$u_0 = \frac{1}{\zeta(t)}, u_1 = \frac{t_1}{\zeta(t)} \cdots u_n = \frac{t_n}{\zeta(t)}. \tag{74}$$

Now

$$\begin{aligned} A' &= \frac{i}{2}(\partial - \bar{\partial})\ln h' \\ &= \frac{i}{2}\frac{\bar{u}_0 du_0 + \bar{u}\cdot du - (u_0 d\bar{u}_0 + u\cdot d\bar{u})}{\bar{u}_0 u_0 + \bar{u}\cdot u}. \end{aligned} \tag{75}$$

The gauge invariant field strength can therefore be written also as

$$F = dA' = \frac{i}{2}\frac{1}{\bar{u}\cdot u}\left(d\bar{u}\cdot du - \frac{\bar{u}\cdot du\, u\cdot d\bar{u}}{\bar{u}\cdot u}\right), \tag{76}$$

where, with a change in notation,

$$\bar{u}\cdot du = \sum_{i=0}^{n} \bar{u}_i du_i \tag{77}$$

etc.

We have introduced gauge transformations as operating on the fiber coordinate as a consequence of a change in the basis vector of the fiber. They are also essential in gluing together the local pieces of the bundle by providing the transition from one open neighborhood to another. For instance, in the neighborhood U_1 we can use local coordinates

$$s_1 = \frac{z_0}{z_1},\ s_2 = \frac{z_2}{z_1}, \cdots s_n = \frac{z_n}{z_1} \tag{78}$$

and write

$$k = |z_1|^2(1 + |s_1|^2 + \cdots |s_n|^2). \tag{79}$$

In the overlap of U_0 and U_1 both z_0 and z_1 are different from zero. The same vector $z_0, z_1 \cdots z_n$ can be said to have fiber coordinate z_0 (for U_0) and z_1 (for U_1). These two are obtained one from the other by the gauge transformation

$$z_1 = g_{10} z_0, \tag{80}$$

where the "transition function"

$$g_{10} = t_1 = \frac{1}{s_1} \tag{81}$$

can be expressed in terms of the local coordinates t of U_0 as well as in terms of the local coordinates s of U_1. In general, in the intersection of U_i and U_j one can use as fiber coordinates ψ_i or ψ_j, related by

$$\psi_i = g_{ij} \psi_j. \tag{82}$$

For consistency the transition functions g_{ij} must satisfy the cocycle conditions

$$g_{ij} g_{ji} = 1 \qquad \text{in} \quad U_i \cap U_j, \tag{83}$$

$$g_{ii} = 1 \tag{84}$$

and

$$g_{ij} g_{jk} g_{ki} = 1 \qquad \text{in} \quad U_i \cap U_j \cap U_k. \tag{85}$$

Finally, observe that the gauge invariant real form B can be written in terms of the homogeneous coordinates as

$$B = \frac{i}{2} \frac{\bar{z} \cdot dz - z \cdot d\bar{z}}{\bar{z} \cdot z} \tag{86}$$

and the curvature as

$$F = dB = i \frac{1}{\bar{z} \cdot z} \left(d\bar{z} \cdot dz - \frac{d\bar{z} \cdot z \; \bar{z} \cdot dz}{\bar{z} \cdot z} \right). \tag{87}$$

Here again, the summation index runs from 0 to n, as in (77).

The similarity of (75) and (76) with (8) and (9) is obvious. In the example of Section 2 the Hilbert space has two complex dimensions, the projective space of rays is $P_1(C)$ and has one complex dimensions ($P_1(C)$ is topologically equivalent to the two–dimensional real sphere S_2). The eigenvalue $E(\lambda)$ corresponding to a point λ in parameter space determines a ray of eigenvectors. Thus, as λ varies, the eigenvalue determines a map from parameter space into $P_1(C)$. More precisely, as we have seen above, this is a map m from a two sphere around the origin in parameter space into the space of rays $P_1(C)$. By this map, the natural bundle over $P_1(C)$ becomes a line bundle over the two–sphere in parameter space.

If we normalize the Hilbert space vectors we obtain a $U(1)$ bundle of phases. Notice that the set of normalized vectors of the two–dimensional Hilbert space is topologically equivalent to a three–sphere S_3. If the map m can be extended to a map from the two–sphere in parameter space into this S_3, the $U(1)$ bundle will be trivial. This would require that it be possible to define globally for all λ the phase of the normalized vectors.

The generalization to a higher dimensional space of rays is obvious. Given an eigenvalue $E(\lambda)$, one has a map from parameter space into the projective space of rays. By this map, the natural line bundle over the space of rays becomes a line bundle over parameter space. If we normalize the Hilbert space vectors, we have a $U(1)$ bundle of phases. Criteria can be given [16] for the nontriviality of this $U(1)$ bundle.

4. Conclusion.

As we have seen in some detail, the non integrable quantum phase has a natural mathematical description in terms of a line bundle. As mentioned in the introduction, the concept of line bundle shows up in a number of other physics problems.

Consider, for instance, the bosonic string. Amplitudes are obtained by performing a functional integration over all world sheets which describe the possible space time evolution of the string. In the Euclidean formulation one must integrate over all Riemann surfaces of different genus. For a given genus g (number of handles) a Riemann surface is determined up to conformal equivalence by $3g-3$ complex parameters y^i, the coordinates of the moduli space. Because of the two–dimensional conformal invariance of the theory, the functional integral reduces to an integral over moduli space. The integrand $Z(y^i, \bar{y}^i)$, called the "partition function", has been the object of intensive study in recent time. In general, it is not really a function, rather it is a section of a potentially nontrivial line bundle having moduli space as base. Only when certain cancellations of anomalies occur, is Z truly a function that can be integrated to give physically acceptable results [12, 13].

A classic problem in the theory of Riemann surfaces, which comes up repeatedly in string theory, is the study of the functions on a given Riemann surface as charaterized by their zeros and singularities. This involves the theory of divisors for which the concept of line bundle is a natural tool. A related problem is that of the classification of different spin structures on a Riemann surface, important for the study of superstrings [11].

In a nonabelian gauge theory with chiral spinors, gauge invariance may be spoiled by the occurrence of anomalies. In the Hamiltonian formulation these anomalies manifest themselves as an anomalous Schwinger term in the commutator of the generators of gauge transformations. These Schwinger terms can be interpreted as the curvature two–form of a suitable line bundle [7, 8].

Finally, let me mention that line bundles over phase space occur as a first step (prequantization) towards the quantization of a dynamical system. If the phase space is topologically non trivial, the line bundle can also be non trivial. Quantization can then be performed consistently only if certain parameters upon which the Hamiltonian depends take quantized values (these parameters can be for instance coupling constants, masses etc.). Typical examples are the value of the magnetic charge of a Dirac monopole and the value of the spin of a particle [6, 17].

I wish to acknowledge numerous instructive conversations with O. Alvarez, I. Singer, H. Sonoda, R. Stora and P. Windey. They have taught me much of what I know on the subject of this lecture.

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T.-Z. Ny

K. C. Chou

INTRODUCTORY REMARKS

Tsi-ze Ny

National People's Congress of China,
Presidium of the Chinese Academy of Sciences

It gives me special pleasure to be here participating in this celebration. Listening to the talks this morning about the discovery of parity nonconservation thirty years ago and this afternoon on theoretical physics fills me with nostalgia about physics and physicists. When I went to Paris to study physics from China in the early twenties, quantum mechanics was still in its infancy and physics in China was nearly non-existent. At that time I was very young, full of confidence and expectation. Nevertheless, I did not anticipate that I would be excited and thrilled by so many great discoveries made in my profession during my lifetime. Since I started science a little earlier than perhaps most of you in this room, in this sense I was even more fortunate than you.

One of these great discoveries is parity nonconservation. It gives me particular pride to see that several of my compatriots were active participants in that revolution: Professor T.D. Lee, whose birthday we are celebrating, Professor C.N. Yang and Professor C.S. Wu.

In 1979, on behalf of the Chinese Academy of Sciences, we invited Professor Lee to give two courses of lectures in physics in Beijing. At that time, he and I discussed the training of future Chinese physicists. One of the outcomes of this discussion was the establishment of the CUSPEA program, which is now in its seventh year. It has brought about 700 graduate students to the U.S. to receive advanced training in physics. Some of the early ones have already gone back to China to teach and to do research.

I have heard that these students have all done quite well, thanks to our American colleagues. I understand that tomorrow afternoon there will be a continuation of this celebration organized by the CUSPEA students, presenting their own scientific papers. I am looking forward to judging for myself how good they really are. The future of physics is in the hands of the younger generation and I am delighted that China is making a significant contribution.

Now I wish to introduce our next speaker, Professor K.C. Chou, Vice President of the Chinese Academy of Sciences.

TIME REVERSAL INVARIANCE AND ITS APPLICATION TO NONEQUILIBRIUM STATIONARY STATES

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Abstract

Generalized free energy of order parameters near nonequilibrium stationary states is shown to exist for systems obeying time reversal invariance. A low frequency fluctuation-dissipation theorem similar in form to that in thermoequilibrium is obtained in this case.

I am very pleased and honored to be here to join so many eminent physicists to celebrate Professor Lee's sixtieth birthday. Professor Lee has not only made many contributions to world science, but has also done much to promote collaborations between the U.S. and China. So, on behalf of Academia Sinica and all Chinese physicists I would like to extend our warmest congratulations to T.D. and Jeannette, and to wish him a very happy birthday and a long creative life ahead.

In 1979, the graduate school in China was reopened after the disastrous turmoil which lasted more than a decade, and scientists started to re-educate themselves. That summer T.D. went to China and gave an intensive course on particles and field theory to about 600 graduate students and scientists gathering from all over China. Almost every day for eight weeks, he lectured three hours in the morning and discussed with the students during lunch and sometimes in the afternoon. It was the first time after years of ignorance that we were able to touch the frontier of physics through excellent lectures. T.D.'s talent as a great scientist and a great teacher was fully displayed and delighted the audience.

Through contact with these brilliant young students, T.D. realized that China, although poor in material production, is rich in human resources. If these young people could be trained at good universities, they certainly would make great contributions not only to the modernization of China, but also to the development of world science. The next year, 1980, T.D. started the CUSPEA project with great enthusiasm and devotion. Over seventy universities in the United States and all universities in China joined the project. Up to now, 704 Chinese students have been sent to study for their Ph.D. degrees in the United States. I am certain it will have far-reaching impact on the development of U.S.-China friendship and scientific collaboration.

I attended some of T.D.'s lectures that summer. I was also motivated by the stimulating atmosphere and vivid discussions among the audience. The ceaseless endeavor of T.D. to find something new has encouraged us to do some work. What I shall talk about in the following is research done at that time, after T.D.'s lecture on CPT and spontaneous CP violation.[1] The results have been published in Chinese.[2] I was interested in laser, a physical system far from thermoequilibrium at that time. We were looking for a generalized free energy to describe processes near nonequilibrium stationary states from a microscopic rather than phenomenological point of view.

Theories based on the Master equation, the Fokker-Planck equation and the Langevin equation were already developed to deal with systems near nonequilibrium stationary states.[3-4] These theories are semiphenomenological in nature. In analogy with statistical mechanics, a generalized free energy was introduced. The minimum of the generalized free energy corresponds to the nonequilibrium stationary state and the curvature at the minimum point determines the linear fluctuation of the system near its stationary state. It was shown in the framework of the Fokker-Planck equation that the existence of generalized free energy can be justified under the assumption of detail balancing.[3]

In a series of papers,[5] we have applied the field theory of closed time path Green's functions (CTPGF) to systems near nonequilibrium stationary states. A time-dependent Ginsburg-Landau equation (TDGL) was derived in the form

$$\gamma_{\alpha\beta} \frac{\partial Q_\beta}{\partial t} = -\frac{\delta F}{\delta Q_\alpha} + J_\alpha \ , \qquad (1)$$

where Q_α , $\alpha = 1,\ldots n$ the order parameters are the average values of some composite operators of the underlying fields. J_α , $\alpha = 1,\ldots n$ are the external sources coupled to Q_α . The generalized free energy F has been shown to exist for systems near thermal equilibrium.

For systems far from thermal equilibrium, does the generalized free energy exist? If it does, what is the condition? The purpose of the present talk is to answer these questions. It will be shown that the time reversal invariance of the underlying field theory is the basis for the existence of the generalized free energy. Since we know that CP is not an exact symmetry, neither is time reversal invariance. As was pointed out first by T.D. Lee, CP and T might be broken spontaneously.[1] If this is true, it will be most promising from the theoretical point of view. Fortunately, the argument leading to the existence of generalized free energy is not affected by the spontaneous breakdown of the time reversal symmetry.

1. Time Reversal Invariance

Consider a system with the Hamiltonian

$$\hat{H}(J) = \hat{H}_0 + \hat{Q}_\alpha \hat{J}_\alpha \quad , \tag{2}$$

where $\hat{H}_0$ is the Hamiltonian without the external source term; $\hat{Q}_\alpha$, $\alpha = 1,\ldots n$ are hermitian composite operators. Under time reversal $J_\alpha(t)$ may change sign

$$J_\alpha(t) \rightarrow \epsilon_{(\alpha)} J_\alpha(-t) \quad , \tag{3}$$

$$\epsilon_{(\alpha)} = \pm 1. \tag{4}$$

In Eq. (3) the index α is not summed.

In the following we shall work in the Heisenberg picture and take $t = 0$ to be the initial time where the Heisenberg picture coincides with the Schroedinger picture. The physical quantities evaluated at time t are independent of the particular initial time chosen.

Time reversal invariance implies the existence of an antiunitary operator R such that for any hermitian dynamical variables

$$\hat{Q}_\alpha(t, J) \rightarrow R\, \hat{Q}_\alpha(t, J)\, R^+ = \epsilon_{(\alpha)}\, \hat{Q}_\alpha(-t, \epsilon J) \, . \tag{5}$$

The state of a statistical system is described by a density matrix $\hat{\rho}(\lambda)$ specified by a set of real parameters λ_α, $a=1,\ldots m$ which may also change sign under time reversal

$$\lambda_\alpha \rightarrow \epsilon_{(\alpha)} \lambda_\alpha \, , \tag{6}$$

$$\epsilon_{(\alpha)} = \pm 1 \ . \tag{7}$$

The state of the system is called time reversal invariant if it satisfies the relation

$$R\,\hat{\rho}(\lambda)\,R^{+} = \hat{\rho}(\epsilon\lambda) \ . \tag{8}$$

If the time reversal invariance is broken spontaneously, there will be some $\lambda_\alpha \neq 0$ with $\epsilon_{(\alpha)} = -1$. In this case, we may live in one world, and do not interact with the time-reversal counterpart because the barrier between the two worlds is infinitely high. Then we can always assume the density matrix satisfies Eq. (8) in the study of dynamical systems living in our world.

The average value of the operators $\hat{Q}_\alpha(t, J)$ is equal to

$$Q_\alpha(t; J, \lambda) = tr\,(\hat{\rho}(\lambda)\,\hat{Q}_\alpha(t, J)), \tag{9}$$

which will be called order parameters in the following. It is easily deduced from Eqs. (5) and (8) for a time reversal invariant system that

$$Q_\alpha(t; J, \lambda) = \epsilon_{(\alpha)}\,Q_\alpha(-t; \epsilon J, \epsilon\lambda). \tag{10}$$

The correlation functions are

$$C_{\alpha_1 \cdots \alpha_\ell}(t_1, \cdots t_\ell ; J, \lambda) = tr\,\{\hat{\rho}(\lambda)\,\hat{Q}_{\alpha_1}(t_1, J) \cdots \hat{Q}_{\alpha_n}(t_\ell, J)\}. \tag{11}$$

Time reversal invariance requires that

$$C_{\alpha_1 \cdots \alpha_\ell}(t_1, \cdots t_\ell ; J, \lambda) = \epsilon_{(\alpha_1)} \cdots \epsilon_{(\alpha_\ell)}\, C_{\alpha_\ell \cdots \alpha_1}(-t_\ell, \cdots -t_1; \epsilon J, \epsilon\lambda). \tag{12}$$

From Eqs. (10) and (12) we see that time reversal invariance relates physical quantities in one world to those in the time reversal counterparts if time reversal symmetry is spontaneously broken.

2. Time-dependent Ginsburg-Landau Equation (TDGL)

In this section we are going to derive TDGL from the field theory of CTPGF and prove the existence of generalized free energy for time reversal invariant systems. Since the relation obtained in the previous section is independent of the particular initial time chosen, we shall take the initial time to minus infinity in the following.

Consider the external source term $J_\alpha \hat{Q}_\alpha (JQ)$ to be a perturbation adiabatically switched on. Any operator $\hat{O}_0$ in the Heisenberg picture of $\hat{H}_0$ will change to

$$\hat{O}(t) = \hat{S}^+ T(S\hat{O}_0(t)) , \tag{13}$$

where $\hat{S}$ is the S-matrix

$$\hat{S} = T(\exp\{ -i \int_{-\infty}^{\infty} J(\tau)\, \hat{Q}(\tau)\, d\tau \}). \tag{14}$$

The $\hat{S}^+$ on the left in Eq. (13) is also necessary to guarantee the causality of the interaction. Equation (13) can be expressed as[6]

$$\hat{O}(t) = T_P(S_P \hat{O}_0(t)) , \tag{15}$$

where

$$S_P = T_P\{ \exp\{-i \int_P J(\tau)\, \hat{Q}(\tau)\, d\tau\}\}. \tag{16}$$

The path of integration P is a closed path starting from $\tau = -\infty$ to $\tau = +\infty$ (positive branch) and back from $\tau = +\infty$ to $\tau = -\infty$ (negative branch). T_P is an ordering operator along the whole path P. It is easily seen that the positive branch of $\hat{S}_P$ corresponds to $\hat{S}$ in Eq. (13) and the negative branch to $\hat{S}^+$.

In statistical mechanics we are interested in average values of physical variables rather than transition amplitudes. The average value is equal to a trace of a density matrix at an initial time t_0 and some Heisenberg operators at the time $t_1, \cdots t_n$ which consists of amplitudes propagating from t_0 to the time of the operators and back. This is why we need an S-matrix along a closed path in statistical physics.

The generating functional for CTPGF is defined to be

$$\begin{aligned} Z[J] &= \exp\{-iW[J]\} \\ &= \mathrm{tr}\{\hat{\rho}\, T_P(\exp\{ -i \int_P J(\tau)\, \hat{Q}(\tau)\, d\tau \})\}. \end{aligned} \tag{17}$$

The external sources $J_\alpha(t)$, $\alpha = 1, \ldots n$ are taken to be different on positive $(J_{+\alpha}(t))$ and negative $(J_{-\alpha}(t))$ branches. They will be put equal at the final step when physical results are evaluated. $W[J]$ is the generating functional for the connected Green's functions whose first derivative gives the order parameters

$$\frac{\delta W[J]}{\delta J_\alpha(t)} = Q_\alpha(t) \ . \tag{18}$$

When the external sources on the two branches are set equal to the physical external source, we have

$$Q_{+\alpha}(t) = Q_{-\alpha}(t) = Q_{\alpha\ ph.}(t) \ , \tag{19}$$

which is the average value of the physical variable $\hat{Q}_\alpha(t)$.

From the definition of the generating functional it follows easily that

$$W[J_+, J_-]\Big|_{J_+ = J_-} = 0 \ . \tag{20}$$

Let

$$\left.\begin{aligned} J_\Sigma &= 1/2(J_+ + J_-) \ , \\ J_\Delta &= J_+ - J_- \ . \end{aligned}\right\} \tag{21}$$

Equation (20) can be rewritten as

$$W[J_\Sigma, J_\Delta = 0] = 0 \ . \tag{22}$$

By differentiation with respect to J_Σ we obtain a series of equalities among Green's functions from Eq. (22). The first equality is

$$\frac{\delta W[J_\Sigma, J_\Delta = 0]}{\delta J_{\Sigma\alpha}(t)} = Q_{+\alpha}(t) - Q_{-\alpha}(t) = 0 \tag{23}$$

coinciding with Eq. (19).

Second order connected Green's functions can be obtained by differentiation

$$G_{P\alpha\beta}(t, t') = \frac{\delta^2 W[J]}{\delta J_\alpha(t)\, \delta J_\beta(t')} \ . \tag{24}$$

In the single time formalism there are four second order Green's functions for each pair $\alpha\beta$, of which only three are independent. They are the retarded

$$G_{r\alpha\beta}(t, t'; J, \lambda) = \frac{\delta^2 W}{\delta J_{\Sigma\alpha}(t)\, \delta J_{\Delta\beta}(t')}$$

$$= -i\theta(t-t')\, \mathrm{tr}\{\hat{\rho}(\lambda)\left[\hat{Q}_\alpha(t, J), \hat{Q}_\beta(t', J)\right]\} \quad , \qquad (25)$$

the advanced

$$G_{a\alpha\beta}(t, t'; J, \lambda) = \frac{\delta^2 W}{\delta J_{\Delta\alpha}(t)\, \delta J_{\Sigma\beta}(t')}$$

$$= i\theta(t'-t)\, \mathrm{tr}\{\hat{\rho}(\lambda)\left[\hat{Q}_\alpha(t, J), \hat{Q}_\beta(t', J)\right]\} \quad , \qquad (26)$$

and the correlation Green's function

$$G_{c\alpha\beta}(t, t'; J, \lambda) = \frac{\delta^2 W}{\delta J_{\Delta\alpha}(t)\, \delta J_{\Delta\beta}(t')}$$

$$= -i\, \mathrm{tr}\{\hat{\rho}(\lambda)\{\hat{Q}_\alpha(t, J), \hat{Q}_\beta(t', J)\}_+\} \quad . \qquad (27)$$

The vertex functional is defined to be

$$\Gamma[Q] = W[J] - \int_p J(\tau)\, Q(\tau)\, d\tau \quad , \qquad (28)$$

where $Q_\alpha(t, J)$ is determined by Eq. (18). It follows from Eqs. (18) and (28) that

$$\frac{\delta \Gamma[Q]}{\delta Q_\alpha(t)} = -J_\alpha(t) \quad . \qquad (29)$$

In terms of the variables

$$Q_{\Sigma\alpha} = (Q_{+\alpha} + Q_{-\alpha})/2 \tag{30}$$

and

$$Q_{\Delta\alpha} = Q_{+\alpha} - Q_{-\alpha} , \tag{31}$$

we have from Eqs. (22) and (28)

$$\Gamma\left[Q_{\Sigma}\, Q_{\Delta} = 0\right] = 0 \tag{32}$$

and

$$\left.\frac{\delta\Gamma\left[Q_{\Sigma}\, Q_{\Delta}\right]}{\delta Q_{\Delta}(t)}\right|_{Q_{\Delta}=0} = -J_{\Sigma}(t) . \tag{33}$$

The vertex functional can be calculated by summing all 1-particle irreducible diagrams. Once this is done, the physical order parameter $Q_{\Sigma\alpha}(t)$ can be determined from Eq. (33). We shall show in the following that TDGL is an approximation of Eq. (33) for systems near stationary states where the motion is slow.

There are four second order vertex functions for each pair $\alpha\beta$ in single time formalism, of which three are independent in the physical region where $Q_{\Delta} = 0$. They are the retarded

$$\Gamma_{R\alpha\beta}(t, t' ; Q, \lambda) = \frac{\delta^2\Gamma}{\delta Q_{\Sigma\alpha}(t)\, \delta Q_{\Delta\beta}(t')}$$

$$= -D_{\alpha\beta} - iA_{\alpha\beta} , \tag{34}$$

the advanced

$$\Gamma_{a\alpha\beta}(t, t' ; Q, \lambda) = \frac{\delta^2\Gamma}{\delta Q_{\Delta\alpha}(t)\, \delta Q_{\Sigma\beta}(t')}$$

$$= -D_{\alpha\beta} + iA_{\alpha\beta} , \tag{35}$$

and the correlation vertex function

$$\Gamma_{c\alpha\beta}(t, t' ; Q, \lambda) = \frac{\delta^2 \Gamma}{\delta Q_{\Delta\alpha}(t)\, \delta Q_{\Delta\beta}(t')} \quad . \tag{36}$$

In Eqs. (34) and (35) $D_{\alpha\beta}$ is called the dispersive part and $A_{\alpha\beta}$ the absorptive part of the vertex.

One can easily deduce the Dyson-Schwinger equations for the second order Green's functions. In matrix form they are

$$\Gamma_r G_r = G_r \Gamma_r = -1 \, , \tag{37}$$

$$\Gamma_a G_a = G_a \Gamma_a = -1 \tag{38}$$

and

$$\Gamma_c = \Gamma_R G_c \Gamma_a \quad . \tag{39}$$

Now we are prepared to derive TDGL for a system near stationary states. In the following we shall assume the physical external source J_Σ to be constant in time and the equation

$$\left. \frac{\delta \Gamma[Q]}{\delta Q_\Delta(t)} \right|_{Q_\Delta = 0} = -J_\Sigma \tag{40}$$

to have constant solution Q_Σ. Then the system is considered to be in a stationary state. The question is whether or not there exists a generalized free energy $F[Q_\Sigma]$ such that Q_Σ is the solution of the equation

$$\frac{\partial F}{\partial Q_{\Sigma\alpha}} = -J_{\Sigma\alpha} \quad . \tag{41}$$

If this is possible we must have

$$\frac{\partial F}{\partial Q_{\Sigma\alpha}} = \left. \frac{\delta \Gamma}{\delta Q_{\Delta\alpha}(t)} \right|_{Q_{\Delta\alpha} = 0} . \tag{42}$$

Then

$$\frac{\partial^2 F}{\partial Q_{\Sigma\beta}\,\partial Q_{\Sigma\alpha}} = \int dt' \left.\frac{\delta^2 \Gamma}{\delta Q_{\Sigma\beta}(t')\,\delta Q_{\Delta\alpha}(t)}\right|_{Q_{\Delta\alpha}=0,\; Q_{\Sigma\beta}(t)=Q_{\Sigma\beta}}$$

$$= \int dt' \, \Gamma_{r\beta a}(t'-t\,;\,Q,\lambda) \quad . \tag{43}$$

Here we have used the time displacement invariance when the external sources are time independent. A well-defined function F can be obtained by integrating Eq. (42) if the order of differentiation can be changed in Eq. (43). Therefore the condition for the existence of the generalized free energy is

$$\int dt' \, \Gamma_{r\beta a}(t'-t\,;\,Q,\lambda) = \int dt' \, \Gamma_{ra\beta}(t'-t\,;\,Q,\lambda)$$

or in Fourier transform

$$\Gamma_{ra\beta}(\omega=0\,;\,Q,\lambda) = \Gamma_{ra\beta}(\omega=0\,;\,Q,\lambda)$$

$$= \Gamma_{aa\beta}(\omega=0\,;\,Q,\lambda) \quad . \tag{44}$$

The last equality in Eq. (44) follows from the relation

$$\Gamma_{ra\beta}(\omega\,;\,Q,\lambda) = \Gamma_{a\beta a}(-\omega\,;\,Q,\lambda) \quad . \tag{45}$$

Equation (44) can also be written in the form

$$\mathcal{A}_{a\beta}\;(\omega=0\,;\,Q,\lambda) = 0. \tag{46}$$

Therefore, vanishing of the zero frequency component of the absorptive part of the vertex function is a sufficient condition for the existence of a generalized free energy.

Our next task is to show that Eq. (46) follows from time reversal invariance. For constant external sources, the order parameters are time independent. We have from Eqs. (10) and (12) that

$$Q_\alpha(J,\lambda) = \epsilon_{(\alpha)} Q_\alpha(\epsilon J, \epsilon\lambda) \tag{47}$$

and

$$G_{r\alpha\beta}(t-t', J, \lambda) = \epsilon_{(\alpha)}\epsilon_{(\beta)} G_{a\alpha\beta}(-t+t'; \epsilon J, \epsilon\lambda). \tag{48}$$

From Eq. (47) we obtain by differentiation with respect to constant $J_{\Sigma\beta}$ the following relation

$$G_{r\beta\alpha}(\omega=0; J, \lambda) = \epsilon_{(\alpha)}\epsilon_{(\beta)} G_{r\beta\alpha}(\omega=0, \epsilon J, \epsilon\lambda) \ . \tag{49}$$

The Fourier transform of Eq. (48) reads

$$G_{r\alpha\beta}(\omega, J, \lambda) = \epsilon_{(\alpha)}\epsilon_{(\beta)} G_{a\alpha\beta}(-\omega; \epsilon J, \epsilon\lambda) \ . \tag{50}$$

From Eqs. (49) and (50) we get

$$G_{r\beta\alpha}(\omega=0; J, \lambda) = G_{a\beta\alpha}(\omega=0, J, \lambda) \ . \tag{51}$$

Since $-\Gamma_r$ and $-\Gamma_a$ are the inverse of G_r and G_a as shown in Eqs. (37) and (38) we obtain finally

$$\Gamma_{r\beta\alpha}(\omega=0; J, \lambda) = \Gamma_{a\beta\alpha}(\omega=0; J, \lambda) \ . \tag{52}$$

Equation (52) is just the condition (46) required. Hence we have shown that generalized free energy exists for time reversal invariant systems in stationary states.

Near the stationary state the order parameters vary slowly with time. We can expand Eq. (29) around the stationary point at time t

$$\left.\frac{\delta\Gamma}{\delta Q_{\Delta\alpha}(t)}\right|_{Q_{\Delta\alpha}=0} = \left.\frac{\delta\Gamma}{\delta Q_{\Delta\alpha}(t)}\right|_{Q_{\Delta\alpha}=0,\ Q_{\Sigma\beta}(t)=Q_{\Sigma\beta}} + \int dt' \frac{\delta^2\Gamma}{\delta Q_{\Sigma\beta}(t')\,\delta Q_{\Delta\alpha}(t)} (Q_{\Sigma\beta}(t') - Q_{\Sigma\beta}(t) + \cdots . \tag{53}$$

To first order approximation in time dependence we neglect higher order terms in the expansion and put

$$Q_{\Sigma\beta}(t') - Q_{\Sigma\beta}(t) = (t' - t)\,\frac{\partial Q_{\Sigma\beta}}{\partial t} \quad . \tag{54}$$

Then Eq. (29) becomes

$$\gamma_{\alpha\beta}\,\frac{\partial Q_{\Sigma\beta}}{\partial t} = \frac{\delta F}{\delta Q_{\Sigma\alpha}} - J_{\Sigma\alpha} \quad , \tag{55}$$

where

$$\gamma_{\alpha\beta} = \int dt'(t'-t)\,\Gamma_{r\beta\alpha}(t'-t, Q, \lambda)$$

$$= \frac{1}{i}\,\frac{\partial \Gamma_{r\beta\alpha}(\omega, Q, \lambda)}{\partial \omega}\bigg|_{\omega=0} \quad . \tag{56}$$

Equation (55) is the TDGL equation for the order parameters $Q_{\Sigma\alpha}$. Time reversal invariance implies also a reciprocity relation for the relaxation matrix $\gamma_{\alpha\beta}$

$$\gamma_{\alpha\beta}(Q, \lambda) = \epsilon(\alpha)\,\epsilon(\beta)\,\gamma_{\beta\alpha}(\epsilon Q, \epsilon\lambda) \quad . \tag{57}$$

This is one kind of Onsager relation generalized to systems near nonequilibrium stationary states.

It was proved in Ref. 2 that both the diffusion matrix

$$\sigma_{\alpha\beta} = \gamma^{-1}_{\alpha\gamma}\,\Gamma_{c\gamma\delta}(\omega = 0, Q, \lambda)\,\gamma T^{-1}_{\delta\beta} \tag{58}$$

and the response matrix

$$L_{\alpha\beta} = \frac{1}{i}\,\frac{\partial G_{r\alpha\beta}(\omega, J, \lambda)}{\partial \omega}\bigg|_{\omega = 0} \tag{59}$$

satisfy the reciprocity relations

$$\sigma_{\alpha\beta}(Q, \lambda) = \epsilon(\alpha)\,\epsilon(\beta)\,\sigma_{\alpha\beta}(\epsilon Q, \epsilon\lambda) \tag{60}$$

and

$$L_{\alpha\beta}(J,\lambda) = \epsilon_{(\alpha)}\epsilon_{(\beta)} L_{\beta\alpha}(\epsilon J, \epsilon\lambda) \tag{61}$$

for systems near stationary states. We shall not discuss it in detail here. The interested reader can consult the original literature.

3. Generalized Fluctuation-dissipation Theorem

We have shown in Ref. 5 that the correlation function G_c can be written in the form

$$G_c = G_r N - N G_a \ , \tag{62}$$

where N is a hermitian matrix related to the quasi particle density distribution. In the thermal equilbrium state

$$N_{\alpha\beta}(\omega, Q, \lambda) = \delta_{\alpha\beta} \operatorname{cth} \frac{\omega}{2T} \ , \tag{63}$$

where T is the temperature. In the low frequency limit

$$\lim_{\omega\to 0} N_{\alpha\beta}(\omega, Q, \lambda) = \delta_{\alpha\beta} \frac{2T}{\omega} \ . \tag{64}$$

Substituting Eq. (62) into the Dyson-Schwinger equation (39) we obtain

$$\Gamma_c = \Gamma_r N - N\Gamma_a = -DN + ND - i(AN + NA) \ , \tag{65}$$

where D and A are the dispersive part and the absorptive part respectively. Equation (65) is the transport equation for the particle density N in a slightly inhomogeneous system.

In a stationary state where the vertex functions depend only on the time difference, Eq. (65) can be written in frequency representation in the following form

$$\Gamma_{c\alpha\beta}(\omega) = -D_{\alpha\gamma}(\omega) N_{\gamma\beta}(\omega) + N_{\alpha\gamma}(\omega) D_{\gamma\beta}(\omega)$$

$$- i(A_{\alpha\gamma}(\omega) N_{\gamma\beta}(\omega) + N_{\alpha\gamma}(\omega) A_{\gamma\beta}(\omega)) \ . \tag{66}$$

It is easily proved that

$$i \sum_{\alpha} \Gamma_{c\alpha\alpha}(\omega)$$

is the quantum fluctuation which has to be positive definite. Therefore we have

$$\mathrm{tr}\,(A(\omega)\, N(\omega)) > 0 \quad . \tag{67}$$

From time reversal invariance we have already shown in Eq. (46) that

$$A_{\alpha\beta}(\omega = 0) = 0 \quad .$$

In the low frequency limit we may put

$$A_{\alpha\beta}(\omega) = \omega a_{\alpha\beta} \quad , \tag{68}$$

where $a_{\alpha\beta}$ is the real part of the relaxation matrix $\gamma_{\alpha\beta}$. For a stable stationary state the eigenvalues of the matrix $a_{\alpha\beta}$ have to be positive definite. Since Eq. (67) holds also at zero frequency, the density matrix N must have a pole at $\omega = 0$. Hence

$$N_{\alpha\beta}(\omega) = \frac{2}{\omega} T^{\mathrm{eff}}_{\alpha\beta} \tag{69}$$

for small frequency. Substituting Eqs. (68) and (69) back into Eq. (66) we get

$$D_{\alpha\gamma}(\omega = 0)\, T^{\mathrm{eff}}_{\gamma\beta} = T^{\mathrm{eff}}_{\alpha\gamma}\, D_{\gamma\beta}(\omega = 0) \tag{70}$$

and

$$\mathrm{tr}\,(\Gamma_c(\omega = 0)) = -4i\, \mathrm{tr}(a(\omega = 0)\, T^{\mathrm{eff}}) \quad . \tag{71}$$

This is the generalized fluctuation-dissipation theorem. In the literature of critical dynamics it is always assumed that

$$T^{\mathrm{eff}}_{\alpha\beta} = T^{\mathrm{eff}} \delta_{\alpha\beta} \quad . \tag{72}$$

In this case we have

$$\Gamma_{c\alpha\beta}(\omega = 0) = -4i\, T^{eff}\, a_{\alpha\beta} \quad . \tag{73}$$

Substituting Eq. (69) into Eq. (62), we obtain another form of the fluctuation-dissipation theorem

$$\frac{\partial G_c}{\partial t} = 2(G_r T^{eff} - T^{eff} G_a)$$

$$= 2i\, T^{eff}(G_r - G_a) \quad . \tag{74}$$

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S. Chandrasekhar

ON INTRODUCING T. D. LEE

S. Chandrasekhar

University of Chicago

Kenneth Clark in his Civilization, A Personal View, commenting on the 'great thaw' that occurred around the year 1100 A.D., remarks that "There have been times in the history of men when the earth seems suddenly to have grown warmer", and that during such times one "feels the air of a Russian spring: in every branch of life--action, philosophy, organization, technology--one finds an extraordinary outpouring of energy, an intensification of existence, and even the incidents of history appear as great heroic dramas."

While Kenneth Clark has in his view the perspective of history in the large, what he says applies in a smaller measure to the history of the institutions of learning.

There was such a period of warming at the University of Chicago in the years after the War. The Russian spring was in the air: I felt warmer walking through the corridors and halls of Eckhart, Ryerson and the Fermi Institute than I have ever felt either before or after. Perhaps several of you assembled here, including T.D. Lee whom we honor today, shared that experience.

For those who were not associated with the University of Chicago at that time, may I explain. The long years of the war had ended. The barricades barring entrance to Eckert and Ryerson had been lifted; and walking through the halls one encountered Enrico Fermi, Gregor Wentzel, Maria Mayer, Arthur Dempster, Samuel Allison, Robert Mullikan, Edward Teller, James Franck, Harold Urey, Willard Libby, Marshall Stone, Saunders MacLane, Shung-shen Chern, Andre Wéil, Antonin Zygmund, and others--and I must not fail to mention the younger faculty among whom were Val Telegdi and Murray Gell-Mann. While the presence of these heroic figures warmed the air, the feeling of the Russian spring came from our students of those days.

Among the students of those years who are present here today are Dick Garwin, Jack Steinberger, Jim Cronin, Jay Orear, Walter Selove and Sam Treiman; and others not present today, but whose names you well know are Murph Goldberger, Geoffrey Chew, Frank Yang, Owen Chamberlain and Marshall Rosenbluth. Certainly the list of our

graduate students of those days includes names fully comparable to the names of the faculty that I read.

I was a silent witness during those years of the Russian spring and I am glad to be here today with some of those who at that time contributed to the 'great thaw' that occurred. I feel particularly privileged to be the chairman of this session when T.D. Lee, a former student now a master physicist, will speak to us on "Happiness is when old friends come from far away."

J. Sandweiss, C. Baltay, M. Mintz
J. Lee, J. Wilson, W.A. Wallenmeyer

J. Sucher, Mr. and Mrs. Speiser, T. D. Lee

HAPPINESS IS WHEN OLD FRIENDS COME FROM FAR AWAY

T. D. Lee

Columbia University

1. Cycles in the Chinese Calendar

In the Chinese calendar, each year is designated by two indices (n, m), where n and m are numbers with

$n + m$ = even,

n = number modulus 12, denoted by

甲, 乙, 丙, 丁, ...

and

m = number modulus 10, denoted by

子, 丑, 寅, 卯, ...

Thus, the whole matrix has 1/2 x 10 x 12 = 60 elements. The sequence begins with

(n, m) = (甲 , 子) ;

the next element is formed by increasing both numbers n and m by one to (n, m) = (乙 , 丑), and then each number is increased by another one to (n, m) = (丙 , 寅), and so on. Instead of the century, China uses 60 years as the cycle.

Because China has no indigenous religion, there is no origin of reference in the Chinese calendar, nothing equivalent to the birth of Christ. Hence, all Chinese years

are labeled cyclically and modulus 60. Naturally, this leads to a cosmological view of an oscillatory universe, rather than that emanating from a big bang.

I was born on the 21st day of the tenth moon of 丙寅, and today is exactly the same 21st day of the tenth moon of 丙寅. For a Chinese, reaching 60 has a special significance, not because one is getting on, which is of course true, but because one is entering a new cycle of 60, and therefore can be allowed to be young again. This is amply demonstrated when I look at my teachers and friends in this room: Professors Ta-You Wu, S. Chandrasekhar, I.I. Rabi, T.Z. Ny, C.S. Wu, R. Serber, G.C. Wick, Pief, Leon, Jack, Bram and many others. All of you are doing very well as youths in your second cycle. The same goes for the one-year-old Sam Treiman. Look at Sid Drell, who is only two months, and already works on the super SLC. Indeed, happiness is when old friends come from far away.

One may wonder whether the title of my talk was taken from one of the fortune cookies during those famous Columbia Chinese lunches. No, it was not. It is a quotation from the first sentence of Confucius' Analects. Although it was spoken 2500 years ago, the sentiment expressed fits this occasion perfectly. I am indeed grateful to all of you for bringing me the present of such immense happiness.

In ancient Chinese tradition, a complete person should be master of six arts. During an occasion like this, he should compose a musical piece for the occasion, or perform on a musical instrument for his friends, or at least write a special poem to thank his guests. Being only a theoretical physicist, I am afraid my limitation is much too obvious. Nevertheless, I wish to express my thanks in this same spirit, and I will try to do the best I can.

Some of you may not know that my Ph.D. thesis was on white dwarfs under Enrico Fermi. That was the period when Fermi was working on the origin of cosmic radiation and nuclear synthesis. Under the sway of Professor S. Chandrasekhar, many of us at Chicago became interested in astrophysics. Since that served very well as a start for my career in the previous cycle, it seems wise to begin my second cycle following a similar path.

2. Two Questions

With Professor Chandrasekhar's permission, I would like to review the well-known Chandrasekhar limit. Consider a white dwarf, or a neutron star, of radius R, mass M, and fermion number N. The gravitational force is balanced by the fermi pressure. From the equipartition of energy we expect, for the equilibrium state, the magnitude of the gravitational energy to be comparable to that of the kinetic energy. For ultra-relativistic fermions, we have

$$\frac{GM^2}{R} \sim \frac{N^{4/3}}{R} \qquad (1)$$

where G is Newton's constant. Let m be the effective mass, defined by

$$N = \frac{M}{m} . \qquad (2)$$

For a neutron star, the fermions are neutrons and m is the neutron mass m_N; for a white dwarf, they are the electrons and $m = 2m_N$, since there are two nucleons per each electron. Combining (1) and (2), one sees that a critical mass M_c exists:

$$M_c \sim \frac{1}{G^{3/2} m^2} .$$

Relating (in units $\hbar = c = 1$)

$$G = \ell_P^{\,2} ,$$

where ℓ_P is the Planck length, given by

$$\ell_P \cong 10^{-33} \text{ cm} ,$$

we find (because $m_N^{-1} \sim 10^{-14}$ cm)

$$M_c \sim \frac{m}{\ell_P^{\,3} m^3} \sim 10^{57} m_N \sim M_\odot . \qquad (3)$$

For the white dwarf, this is the well-known Chandrasekhar limit

$$M_c \cong 1.4 M_\odot .$$

For the neutron star, because of general relativity and nuclear forces, M_c is somewhat smaller than 2^2 times the white dwarf limit (as would be indicated by (3)); it is commonly accepted as $\lesssim 5 M_\odot$, depending on the physical assumptions.[1-2]

For $M < M_c$, the right-hand side of (1) is bigger than its left-hand side; therefore the fermi pressure will make the star expand; a part of the star would then consist of non-relativistic fermions, which leads to a nonzero R. For $M > M_c$, the gravitational force dominates, which makes the star collapse. For M bigger than $1.4 M_\odot$ but less than $M_c \lesssim 5M_\odot$, white dwarfs cease to exist; instead, one

has a neutron star. For M bigger than M_c of the neutron star, the solution becomes singular ($R = 0$). The star collapses into a black hole. This relatively low critical mass M_c has been used as a criterion for the observation of black holes.

Let us ask two questions:

(i) Can the critical mass M_c for a black hole be $> 5\ M_\odot$, or $\gg 5\ M_\odot$?

(ii) Can there be a non-singular black-hole solution with x, y, z and t each varying from $-\infty$ to ∞ ?

Perhaps I should first comment on the second question. Usually, a black hole (with zero angular momentum) is described by the Schwarzschild solution:

$$ds^2 = -(1 - \frac{2GM}{\rho})\, dt^2 + (1 - \frac{2GM}{\rho})^{-1} d\rho^2 + \rho^2 (d\theta^2 + \sin^2\theta\, d\phi^2) \ . \qquad (4)$$

Note that the solution is valid for $\rho > 2GM$ and t varies from $-\infty$ to ∞. The analytical extension to $\rho < 2GM$ leads to a "time-like" ρ, since $g_{\rho\rho}$ is now < 0. As shown in Figure 1, there are shaded space-time regions that are excluded from the Schwarzschild solution. Hence, the Schwarzschild solution is <u>not</u> a complete solution of the field equation, just as in electrostatics $1/r$ is not a complete solution of the Laplace equation (but a solution of the Poisson equation).

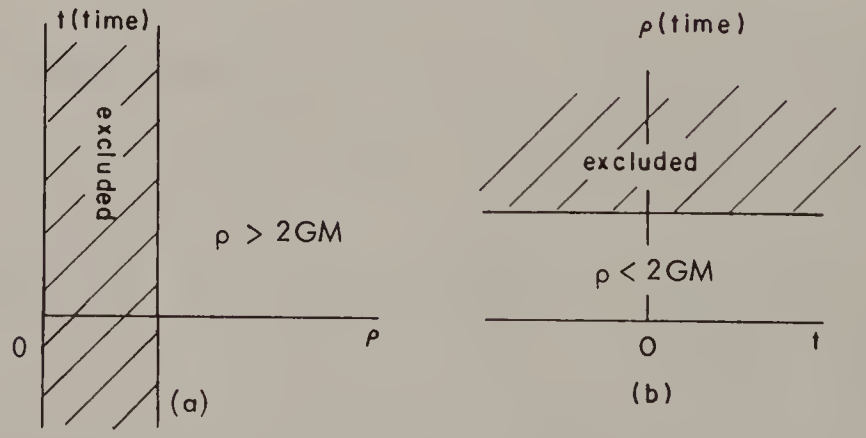


FIGURE 1.

We now turn to the first question. As we shall see,

it is (theoretically) possible to have a quark star with M_c much bigger than $5M_\odot$, without being a black hole.

Recent progress in particle physics points out the importance of nonlinearity and coherence in the realm of 10^{-13}-10^{-17} cm. This is exemplified by the quark-confinement in QCD, which suggests that hadrons form bubbles, or bags, in a QCD vacuum, and that the coherence of the vacuum can be effectively characterized by a scalar field. From the electroweak theory, we learn that all symmetry-breaking phenomena can be described by degenerate vacua through the use of, again, the Higgs scalar field. Once there exists coherence in microscopic distance, it seems reasonable that such coherence can be accumulated and extended to macroscopic and even to astronomical distances. As we shall see, this can lead to an affirmative answer to our first question.

3. Nontopological Soliton[3-5]

To illustrate the basic mechanism, consider the following example of a nontopological soliton, first without gravity. The theory contains an additive quantum number N (like the baryon number) carried by either a spin 1/2 field ψ, or a spin 0 complex field ϕ, with its elementary field quantum having $N = \pm 1$. (If one wishes, one may think of ψ as the quark field.) In addition, there is a scalar field σ. Take, as a first example, the self-interaction of σ to be the typical degenerate vacuum form (in units $\hbar = c = 1$):

$$U(\sigma) = \tfrac{1}{2} m^2 \sigma^2 \left(1 - \frac{\sigma}{\sigma_0}\right)^2 . \tag{5}$$

We may assign $\sigma = 0$ to the normal vacuum state, and $\sigma = \sigma_0$ to the (abnormal) degenerate vacuum state. (Theories of this type have been studied in the literature, e.g., in connection with the spontaneous T violation,[6,7] the abnormal nuclear model,[3] the bag model[8,9] and the Higgs mechanism.[10]) The soliton contains an interior in which $\sigma \cong \sigma_0$, a shell of width $\sim m^{-1}$, over which σ changes from σ_0 to 0, and an exterior that is essentially the vacuum. The N-carrying field ψ, or ϕ, is confined to the interior; this produces a kinetic energy E_k (assuming for simplicity that the mass of ψ, or ϕ, is zero when $\sigma = \sigma_0$, but nonzero when $\sigma = 0$)

$$E_k \sim \begin{cases} N^{4/3} R & \text{for fermions} \\ N/R & \text{for bosons.} \end{cases} \tag{6}$$

The shell contains a surface energy

$$E_s = s R^2$$

where s is the surface tension, related to σ_0 and m by

$$s \sim m \sigma_0^2 \quad .$$

The radius R can be calculated by minimizing the total energy $E = E_k + E_s$. Setting $\partial E / \partial R = 0$, we have the equipartition

$$E_k = 2E_s \quad .$$

Hence, the soliton mass M (which is the minimum of E) can be written as

$$M = 3E_s = 3s R^2 \quad , \tag{7}$$

the total conserved particle number N is related to M by

$$M \propto \begin{cases} N^{8/9} & \text{for fermions} \\ N^{2/3} & \text{for bosons.} \end{cases}$$

Because the exponent of N is < 1, when N is large the soliton mass is always less than that of the free particle solution, and that insures its stability against decaying into N free particles. But, is there a limit to this stability?

4. Soliton Stars[11-14]

To find the upper limit, we must include the gravitational field. For configurations with the soliton radius R much greater than the Schwarzschild radius $2GM$, the effects of gravity can be treated as a perturbation. Gravity becomes important when R becomes of the same order as $2GM$. Thus the critical mass M_c may be estimated by simply equating R with the Schwarzschild radius

$$R \sim 2GM_c \quad ,$$

which leads to, because of (7)

$$M_c \sim \frac{1}{G^2 s} \quad .$$

Since Newton's constant G is the square of the Planck length $\ell_P \cong 10^{-33}$ cm, whereas a typical Higgs-like field σ may have $\sigma_0 \sim m$ (with m^{-1} much less than ℓ_P), we estimate

$$M_c \sim (\ell_P m)^{-4} m \quad . \tag{8}$$

For example, if m is 300 GeV, we have $M_c \sim 10^{12} M_\odot$ and $R \sim 1$ light month. Hence we find the answer to our first question: depending on the physical theory, the critical mass for a hadron star to become a black hole can be much greater than $5M_\odot$. Such cold stable stellar configurations are called soliton stars.

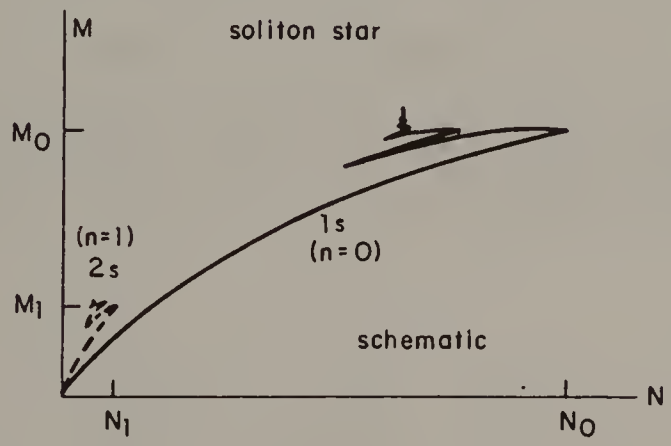


FIGURE 2.

In Figure 2, we plot schematically the M vs. N curve for the 1s state (solid curve) and for the 2s state (dotted curve). The 1s state refers to the $n = 0$ solution (i.e., zero-node, or zero-minimum at any finite radius) of the σ-field. The 2s state refers to the $n = 1$ solution (i.e., one minimum at some finite radius) of the field. The zigzag features of each of the 1s, 2s, 3s, ... curves are independent of the statistics of the particles, but are characteristics of the type of nonlinear equations with which we have to deal. (A linear equation, even with weak gravity, will make M vs. N a monotonic curve for each ns orbit.)

Here, for each given n, there is a maximum $N = N_n$ (with a corresponding mass M_n), beyond which there is no solution. For $N < N_n$, depending on N we may have one solution, or two, or three, . . . , infinite solutions. Furthermore,[15]

$$M_n \cong \frac{1}{2n+1} M_0$$

and

$$N_n \cong \left(\frac{1}{2n+1}\right)^2 N_0 \ . \qquad (9)$$

Thus, only the lowest 1s solution is stable. For $M > M_0$ there is no (spherically symmetric) solution. The critical mass M_c is therefore M_0.

When $M = M_0$, the radius R is $\sim (M_c/s)^{1/2} \neq 0$. Unlike the case of the Chandrasekhar limit, the solution is not singular. Consequently, it is possible to find some non-singular solutions for the black holes when $M > M_0$ (and $N > N_0$). As we shall see, this leads to the answer to our second question: what happens in the excluded region (shown in Figure 1(a)) of the Schwarzschild solution?

5. Soliton Black Holes

As shown in Refs 13 and 14, for $N > N_c$, a new type of solution exists: these are the black-hole solutions. The relation between the mass M and the particle number N takes on a different form, illustrated in Figure 3.

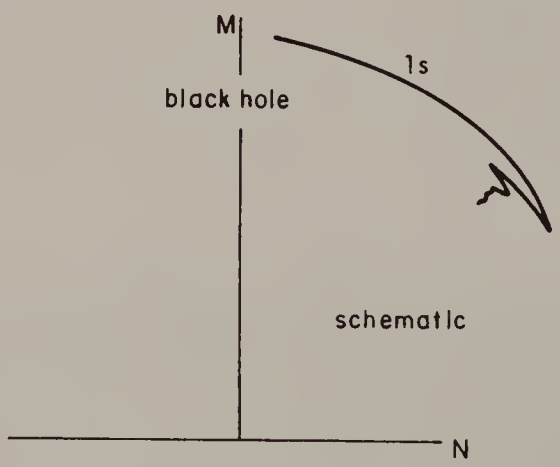


FIGURE 3.

It is most convenient to adopt the isotropic coordinates

$$ds^2 = -e^{2u} dt^2 + e^{2v}(dr^2 + r^2 d\theta^2 + r^2 \sin^2\theta \, d\phi^2) \qquad (10)$$

where, for the spherically symmetric solution, u and v depend only on the radius r. Let $2\pi\rho$ be the circumference of a two-sphere. From (10), we see that

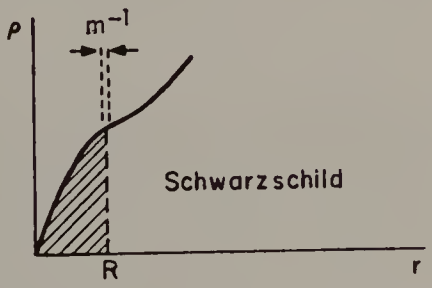
$$\rho = r\, e^{v} \quad . \tag{11}$$

The dependence of ρ on r is plotted schematically in Figures 4(a) and 4(b); the shaded region refers to the star (with nonzero matter density and Higgs field $\sigma \cong \sigma_0$, the false vacuum). At $r = R + O(m^{-1})$, the σ-field changes from σ_0 to 0 over a distance $\sim m^{-1}$. Outside the surface, $r > R + O(m^{-1})$, the matter density becomes zero, and the metric is determined by the Schwarzschild solution:

$$e^{u} = \frac{r-a}{r+a} \quad , \qquad e^{v} = \left(\frac{r+a}{r}\right)^{2} \tag{12}$$

where $a = \frac{1}{2}GM$ is the "Schwarzschild" radius in the isotropic coordinates.

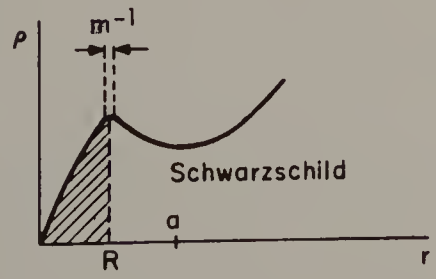
For a soliton star, R is $> a$, and ρ is a monotonic function of r, shown in Figure 4(a).



soliton star
(schematic)

$R > a \equiv \frac{1}{2}GM$

(a)



soliton black hole
(schematic)

$R < a = \frac{1}{2}GM$

(b)

FIGURE 4.

For a soliton black hole, R is $< a$. Although $d\rho/dr$ is > 0 inside the star, outside the star ρ is no longer a monotonic function. From (11) and (12), we see that outside the star,

$$\rho = a\left(\sqrt{\frac{r}{a}} + \sqrt{\frac{a}{r}}\right)^2 ; \qquad (13)$$

hence, $d\rho/dr$ is negative for $R < r < a$ (inside the horizon) and positive for $r > a$ (outside the horizon). The horizon is located at $r = a$ (i.e., $\rho = 2GM$). Notice that the inside and outside regions of the horizon are now related "space-like" to each other (since the metric is time-independent); thus, our solution covers the excluded region shown in Figure 1(a).

Note also that for the black-hole solution

$e^u > 0$ for $r > a$ (outside the horizon)

but

$e^u < 0$ for $r < a$ (inside the horizon).

Consequently, a time-translation induces a line element

$$ds = e^u\, dt$$

which changes its sign across the horizon. If one wishes, one may regard the flow of time as having opposite signs on the two sides of the horizon. (Here, a time-translation is more like an angular rotation, with time as the angle and the horizon as the fixed point.) Inside the star (therefore also inside the horizon) $r < R - O(m^{-1})$ the function e^u is < 0, but $e^v > 0$. However, since Einstein's equation depends only on e^{2u} and e^{2v} (which are both positive) and $d\rho/dr$ is positive (as shown in Figure 4(b)), there is nothing remarkable. Outside the star, the Schwarzschild solution (12) applies. Depending on whether one is inside or outside the black hole, the situation changes drastically: in the region outside the star but inside the black hole, the circumference $2\pi\rho$ of a 2-sphere decreases with increasing r, as mentioned before. Since $-g_{tt} = e^{2u}$ and $g_{rr} = e^{2v}$ are all positive and are exactly the same as the usual ones in an empty space outside a black hole, an observer in this region, $a > r > R + O(m^{-1})$, would feel a gravitational force in the direction away from the star (i.e., in the direction of increasing r, or decreasing ρ) as if there could be a gravitational repulsion from the matter distribution (inside the star). Hence, he might conclude that he is "outside" a black hole. Somehow, in spite of the presence of a star located at $r < R$ (which is within his horizon), to him the "real" black hole seems to lie

in the opposite direction. In any case, he might be perplexed. For $r > a$, the situation becomes normal again, and we have the usual description of an empty space outside a black hole.

6. Latitude of the Model

If we take, instead of (5), an MIT-bag-like potential which gives the false vacuum a higher potential than the real vacuum, in the absence of the gravity the soliton mass would be given by, instead of (6)-(7),

$$M = sR^2 + pR^3 + \begin{cases} N^{4/3}/R & \text{for fermions} \\ N/R & \text{for bosons.} \end{cases} \tag{14}$$

Because $\partial M/\partial R = 0$, we have

$$2sR^2 + 3pR^3 = \begin{cases} N^{4/3}/R \\ N/R \end{cases}$$

and therefore

$$M = 3sR^2 + 4pR^3 .$$

Next, we turn on the gravitational field. The critical mass M_c can again be estimated by setting $R \sim GM_c$. This gives

$$1 \sim 3s\, G^2 M_c + 4p\, G^3 M_c^2$$

or

$$M_c \sim \frac{1}{3s\, G^2} \frac{2}{1 + \sqrt{1 + \xi^2}} \tag{15}$$

where

$$\xi = \frac{4}{3s} \sqrt{\frac{p}{G}} .$$

Hence, when $p = 0$ and $s \neq 0$ we have $\xi = 0$ and $M_c \sim 1/sG^2$ as given before by (8). If $s = 0$ but $p \neq 0$ then $M_c \sim 1/p^{1/2} G^{3/2}$, which has the same power of G as the Chandrasekhar limit (3). Consequently, there is an enormous latitude of M_c for a soliton star, which can vary from a galactic mass to a solar mass.

What happens when p and s are both zero? As we shall see, in that case there is an equilibrium solution for any mass (at least classically).

7. Mini-soliton Star

Consider the simple theory consisting of only a "free" spin 0 complex field ϕ of mass $m \neq 0$, plus gravity. There is again a conserved additive quantum number N. For $N \neq 0$, ϕ must be time-dependent. It can be shown that for the lowest energy state, ϕ depends harmonically on time:

$$\phi = \sigma(r)\, e^{i\omega t} ,$$

for the spherically symmetric solutions. Let n be the number of nodes of σ (in r). For the 1s state ($n = 0$), we have again the typical zigzag M vs. N relation shown by the solid curve in Figure 5, which has a maximum particle number N_0 and a corresponding mass M_0. One may estimate M_0 by realizing that the radius R is now only of microscopic dimension (even though N is >> 1)

$$R \sim m^{-1} .$$

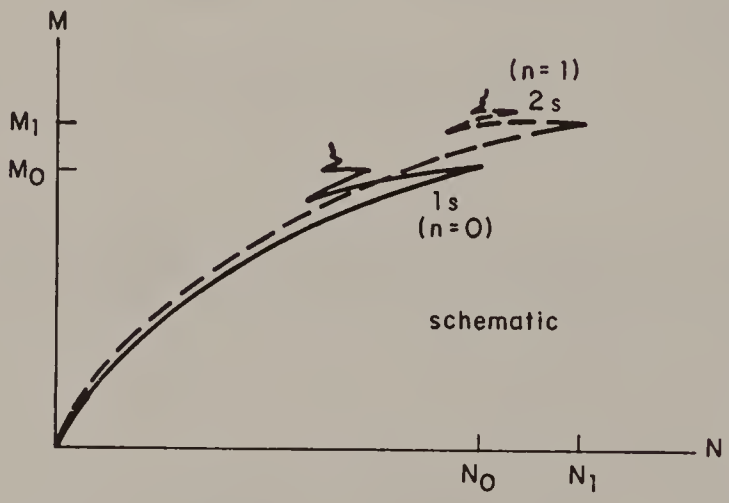


FIGURE 5.

Setting $R \sim GM$, one finds the critical mass (for the 1s states) to be

$$M_0 \sim (\ell_p m)^{-2} m .$$

In the example of $m \sim 300$ GeV, M_0 is $\sim 10^9$ kg, the radius is $\sim 6 \times 10^{-17}$ cm and the corresponding density is extremely high, $\sim 10^{43}$ times that of a neutron star! Because of the smallness of its size, we call such a configuration a mini-soliton star.

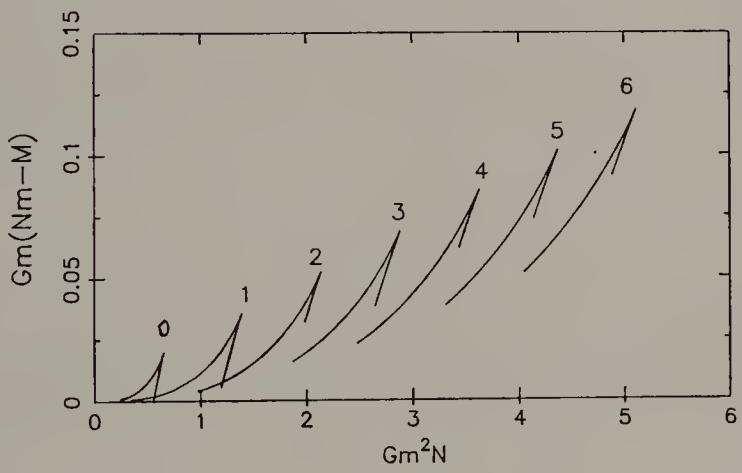
In Ref. 14, it is shown that for any fixed number of nodes n, the relation between M and N always displays the typical zigzag form shown in Figure 5 and Figure 6(a)), with a maximum particle number N_n and a corresponding mass $M_n < N_n m$, which insures stability against decaying into N free particles. The interesting feature is that, in contrast to the soliton star,

$$M_n \cong \frac{n+1}{Gm} \tag{16}$$

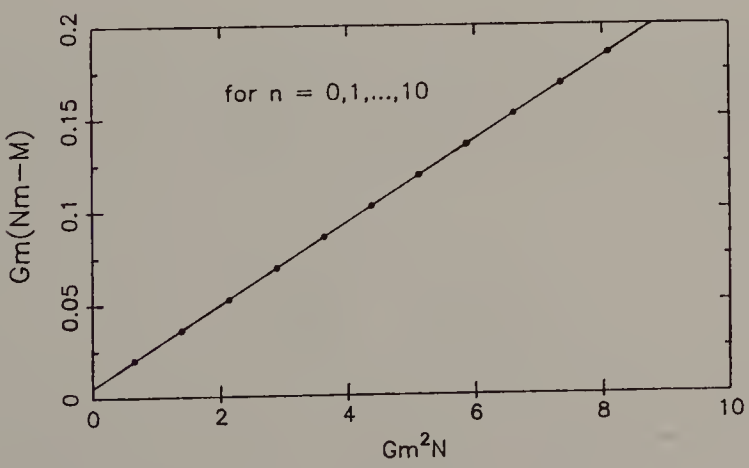
and

$$\lim_{n \to \infty} M_n = \infty \quad .$$

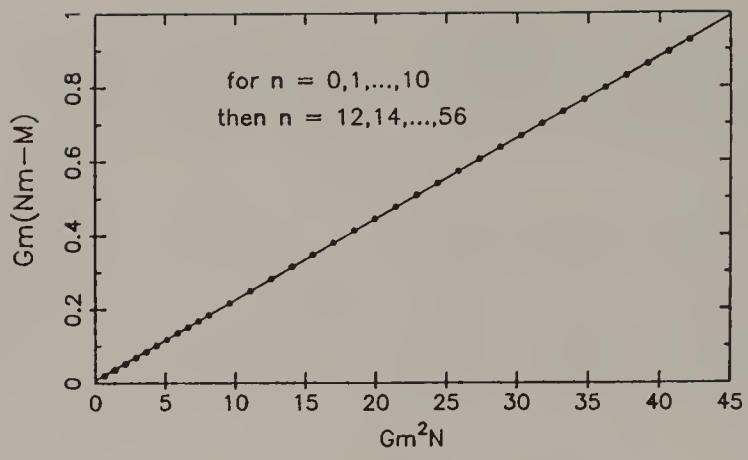
Consequently, there is no upper limit in mass for the equilibrium solutions of the mini-soliton star.



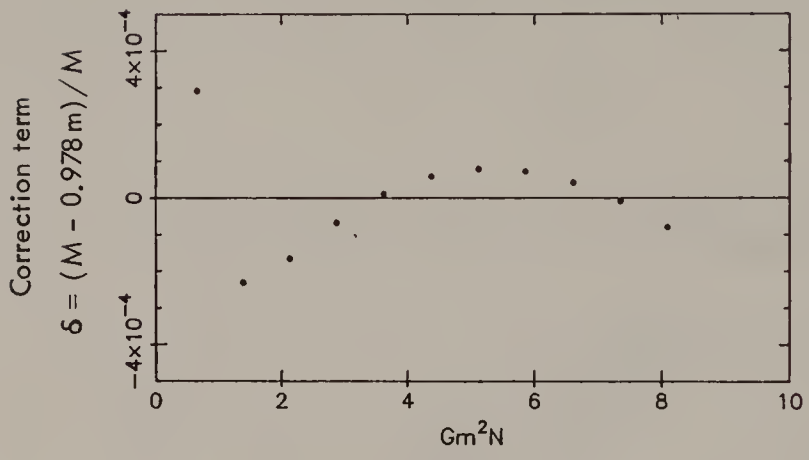
6(a)



6(b)



6(c)



6(d)

FIGURE 6.

As shown in Figure 6(a)-6(d), to a very good approximation, the ratio M_n divided by N_n is a constant:

$$\frac{M_n}{N_n} \cong (1 - 0.022)\, m \ . \tag{17}$$

Equation (16) can be understood by observing that the wavelength of the (n+1)s orbit is $\sim R/(n+1)$. Putting N relativistic bosons in the same (n+1)s orbit and equating the magnitudes of their kinetic energy with their gravitational energy, we have

$$\frac{(n+1)\, N}{R} \sim \frac{GM^2}{R} \tag{18}$$

which leads to (16).

8. Conclusion

Nonlinear field theories have been found to be of importance in all elementary particle interactions: QCD, the electroweak theory, GUT, etc. Many of their physical properties are still in the developing stage. For stellar configurations, although the nonlinearity of gravitation is fully recognized through general relativity, that of the matter field is far from adequately explored. The simple examples given here are only meant to illustrate the physical richness of this exciting new domain.

Anyone at the end of his cycle must experience the singular impact of strong and mixed emotions. But I am more fortunate because I have all my friends here, some from very far away. Being with you at this crucial moment gives me confidence to enter my new cycle. It is indeed a unique privilege and a great comfort to have known you, and I thank you all.

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PART 2

- Reminiscences -

T. D. Lee

Tsung-Dao and Jeannette Lee

REMINISCENCES*

T. D. Lee

Early Chicago Days

Forty years ago, through Professor Ta-You Wu, I received a Chinese Government fellowhsip which enabled me to come to the United States to further my study in physics. That rare opportunity which he offered me changed my life. Among all the relevant factors for creativity,"luck" is perhaps the most important; yet by its very nature it is also the least understood. While it is not possible to order a chance happening, probability can be improved, at least in a statistical sense. Appreciation of the important chance that I had in 1946 led me to organize the CUSPEA program in recent years, so that similar good fortune might come to others.

I came to the U.S. in September 1946 with an academic record of only two years undergraduate training. However, I was already familiar with classical physics and knew some quantum mechanics. I felt well-prepared for graduate study. But at that time to enter graduate school without a college degree was almost impossible, except at the University of Chicago which was willing to take people without a formal degree provided they had read the great books selected by President Hutchins. Luckily for me, the system was so fluid that it was possible to be admitted even if one had not heard of the great books. I convinced the admissions officer that I was quite knowledgeable in the oriental equivalent of such classics (Confucius, Mencius, Laotse, etc.), which she accepted without verification.

Another happy surprise for me was to discover that the Chicago physical science departments right after the war were the best in the world, with S. Chandrasekhar, E. Fermi, J. Mayer, M. Mayer, R. Mulliken, E. Teller, H. Urey and W. Zachariasen (later G. Wentzel also joined the staff) on their faculty, and an equally impressive student body which included H. Agnew, J. Anderson, O. Chamberlain, G. Chew, R. Garwin, M. Goldberger, J. Lord, J. Orear, M. Rosenbluth, W. Selove, J. Steinberger, R. Sternheimer, H. Ticho, S.Treiman, H. Wilcox, L. Wolfenstein and C.N. Yang.

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The first thing I did after my arrival was to read the university catalog. As I recall, it said the department of physics was only interested in exceptional students. It did not encourage students to take courses; however, for those who needed guidance, courses were also provided. I thought to myself that that was really the proper style of a great university. How unlike Southwest Associated University in Yunnan where students absolutely had to take courses. Nevertheless, I did register for quantum mechanics with Teller, electromagnetic theory with Zachariasen and, later, statistical mechanics with both Mayers. By attending those classes I felt I was betraying the secret that I was not an exceptional student. However, that feeling was soon dissipated by my observing that there were many other students in these classes.

In any case, I was quite elated when sometime later I was asked to attend the evening classes of Fermi (which were by invitation only). It was there that I had my first glimpse of Fermi in action. The subject matter ranged over all topics in physics. Sometimes he would randomly pull a card out of his file, on which usually a subject title was written with a key formula. It was wonderful to see how in one session Fermi could start from scratch, give the incisive estimate and arrive at the relevant formula and the physics that could be derived from it. The freedom with which he moved from field to field was an inspiration to watch.

At one time, Fermi happened to pull out his index cards on group theory, which contained only titles, all listed alphabetically. He then started to lecture on Abelian group first, affine correspondence second, central of a group third, and then character of a group, etc. Some of us were a bit confused by this unorthodox approach. Fermi said, "Group theory is merely a compilation of definitions, and alphabetical order is as good as any." In spite of his assurance, students like myself who were not able to apply the permutation group fast enough to change the order had some difficulty.

In those early Chicago days, Frank (C.N.) Yang and I became close friends. Yang was extremely bright. Both of us were very young, and had an enormous curiosity towards practically all subjects rational. Often we would have different ideas and opinions and occasionally our discussions would become quite animated. That added a great deal of life to our student days.

Another thing whch comes to my mind about student life in those times is the square-dancing parties at the Fermis'. They were my first introduction to occidental culture in this country. Enrico's dancing, Laura's punch and Harold Agnew's energetic calling of do-si-do all made indelible marks on my memory.

In the spring of 1948, after passing the basic examination, I began my research under Fermi. The relationship

between Fermi and his students was quite personal. I would see him regularly. Usually we had lunch together in Commons, often with his other students. At that time, Fermi was interested in the origin of cosmic radiation and nuclear synthesis. He directed me first towards nuclear physics and then into astrophysics. Quite often he would mention a topic and ask me if I could think and read about it, and then "give him a lecture" the next week. Of course, I obliged and usually felt very good afterward. Only much later did I realize that this was an excellent way of guiding the student to be independent.

Fermi fostered a spirit of self-reliance and intellectual independence in his students. One had to verify or derive all the formulas that one used. At one point I was discussing with him the internal structure of the sun; the coupled differential equations of radiative transfer were quite complicated. Since that was not my research topic, I did not want to devote too much time to tedious checking. Instead, I simply quoted the results of well-known references. However, Fermi thought one should never accept other people's calculations without some independent confirmation. He then had the ingenious idea of making a specialized slide rule designed to deal with these radiative transfer equations:

$$\frac{dL}{dr} \propto T^{18} \quad \text{and} \quad \frac{dT}{dr} \propto L/T^{6.5} \qquad (1)$$

(where L is the luminosity and T the temperature). Over a week's time, he helped me to produce the magnificent six-foot-seven-inch slide rule in the photograph with 18 log x on one side and 6.5 log x on the other. With that, even

Figure 1. A dedicated slide rule for the internal temperature distribution of a main-sequence star, circa 1948.

integration became fun and I was able to complete the checking quickly and then move on to a different topic for discussion. This unique experience made a deep impression on me. Even now, sometimes when I encounter difficulties, I try to imagine how Fermi might react under similar circumstances.

In 1948, Jack Steinberger, another student in Fermi's lab, was doing experimental work on the e-spectrum in the decay of $\mu \rightarrow e + \cdots$. We talked a lot about his measurements which indicated that μ-decay, like β-decay, involves four fermions. I became quite interested in that, and so did Rosenbluth and Yang, both students of Teller. Are there other interactions besides β-decay that can be described by Fermi's theory? The three of us decided to make a systematic investigation.

We found that if μ-decay and μ-capture were described by a four-fermion interaction similar to β-decay, all their coupling constants appeared to be of the same magnitude. This began the universal Fermi interaction. We then went on to speculate that, in analogy with electromagnetic forces, the basic weak interaction could be carried by a universally coupled intermediate heavy boson, which I later called $W^{\pm}$ for weak. Naturally we told Fermi of our discoveries, and he was extremely encouraging.

One serious difficulty that faced us was how to generate such a universally coupled intermediate boson from symmetry considerations. In order to have short-range interactions and to escape detection, the boson must be massive and unstable, with a very short lifetime. However, other universally coupled quanta, like photons and gravitons, are all massless and stable. In addition, because of parity conservation, it was difficult at that time to understand why there were both Fermi and Gamow-Teller interactions in β-decay. We made no progress in this direction, and procrastinated in writing it up. At the end of December 1948, Fermi called us into his office and said he had just received two preprints by Tiomno and Wheeler[2] which also discussed the universal weak interaction. He insisted that we should immediately publish whatever we had; furthermore, he would send a copy to Wheeler with a letter saying it was independently done some months earlier. We were quite touched by his thoughtfulness.

After this excursion into particle physics, I went back to astrophysics full-time and finished my thesis on white dwarfs under Fermi at the end of 1949. With Fermi's recommendation, Chandrasekhar kindly extended an invitation to me to work with him at Yerkes Observatory for eight months. After that I went to Berkeley for a year, then to the Institute for Advanced Study, and joined Columbia University in 1953.

Parity Violation

In the mid-1950's, through the work of Dalitz and others, the θ-τ problem became the major puzzle in particle physics. Why should two spin-zero particles of different parity and different decay mode have the same mass and the same lifetime?

My first efforts were all on the wrong track. In the summer of 1955, Jay Orear and I proposed[3] a scheme to explain the θ-τ puzzle within the bounds of conventional theory. We suggested a cascade mechanism, which turned out to be incorrect.

The idea that parity is perhaps not conserved in the decay of θ-τ flickered through my mind. After all, strange particles are by definition strange, so why should they respect parity? The problem was that, after you say parity is not conserved in θ-τ decay, then what do you do? Because if parity nonconservation exists only in θ-τ, then we already have all the observable facts, namely the same spin-0 particle can decay into either 2π or 3π with different parity. I discussed this possibility with Yang, but we were not able to make any progress.[4] So we instead wrote papers on parity doublets, which was another wrong try.[5]

In 1956, Steinberger and others were conducting extensive experiments on the production and decay of the hyperons Λ^0 and Σ^- (which are, like θ-τ , also strange particles):

$$\pi^- + p \rightarrow \begin{cases} \Lambda^0 + K^0 \\ \Sigma^- + K^+ \end{cases} \qquad (2)$$

and

$$\left.\begin{matrix} \Lambda^0 \\ \Sigma^- \end{matrix}\right\} \rightarrow \pi + N \ . \qquad (3)$$

The dihedral angle ϕ between the production plane and the decay plane is of importance for the determination of the hyperon spin.

Let $\vec{\pi}$, $\vec{\Lambda}$, and $\vec{N}$ be the momenta of π , Λ in (2) and N in (3), all, say, in the respective center-of-mass systems of the reactions. The normal to the production

plane is parallel to $\vec{\pi} \times \vec{\Lambda}$, and that to the decay plane is parallel to $\vec{N} \times \vec{\Lambda}$. Hence, the dihedral angle ϕ is defined through its cosine:

$$\cos \phi \propto (\vec{\pi} \times \vec{\Lambda}) \cdot (\vec{N} \times \vec{\Lambda}) \quad . \tag{4}$$

Its distribution is

$$\begin{aligned} D(\phi) &= 1 && \text{if the hyperon-spin is } 1/2 \\ &= 1 \times \alpha \cos^2 \phi && \text{if the hyperon-spin is } 3/2, \end{aligned} \tag{5}$$

etc. By this definition, ϕ varies from 0 to π. Furthermore, $D(\phi)$ is identical to $D(\pi - \phi)$. At the Rochester Conference in early April 1956, Steinberger gave a talk and plotted his data on $D(\phi)$ with ϕ varying from 0 to π. However other physicists, W.D. Walker and R.P. Shutt, plotted the sum $D(\phi) + D(\pi - \phi)$; in this way ϕ can only vary from 0 to $\pi/2$. A day or two after the conference, Steinberger came to my office to discuss a communication from R. Karplus; Karplus asked why Steinberger did not also restrict ϕ from 0 to $\pi/2$, since the total number of events was (at that time) quite limited, and a folding of $D(\pi - \phi)$ onto $D(\phi)$ would increase the experimental sensitivity of the spin determination.

The dihedral angle, as defined by expression (4), has nothing to do with parity, since it is a scalar. In the course of the discussion, it occurred to me that there was another way to view this angle: if one modifies the definition of ϕ to be not just the dihedral angle, but also the angle of rotation around the Λ momentum vector, which is the intersection of these two planes, then the range of ϕ can be increased from 0 to 2π. That is, in addition to expression (4), one introduces the pseudoscalar

$$\sin \phi \propto (\vec{\pi}_{\perp} \times \vec{N}_{\perp}) \cdot \vec{\Lambda} \quad , \tag{6}$$

where $\vec{\pi}_{\perp}$ and $\vec{N}_{\perp}$ refer to the components of $\vec{\pi}$ and $\vec{N}$ perpendicular to $\vec{\Lambda}$, as shown in Figure 2.

If parity is not conserved in strange particle decays, there could be an asymmetry between events with ϕ from 0 to π and those with ϕ from ϕ to 2π. <u>This is the missing key</u>! The idea of parity violation could now be applied to systems beyond θ-τ and, thereby, become a verifiable hypothesis.

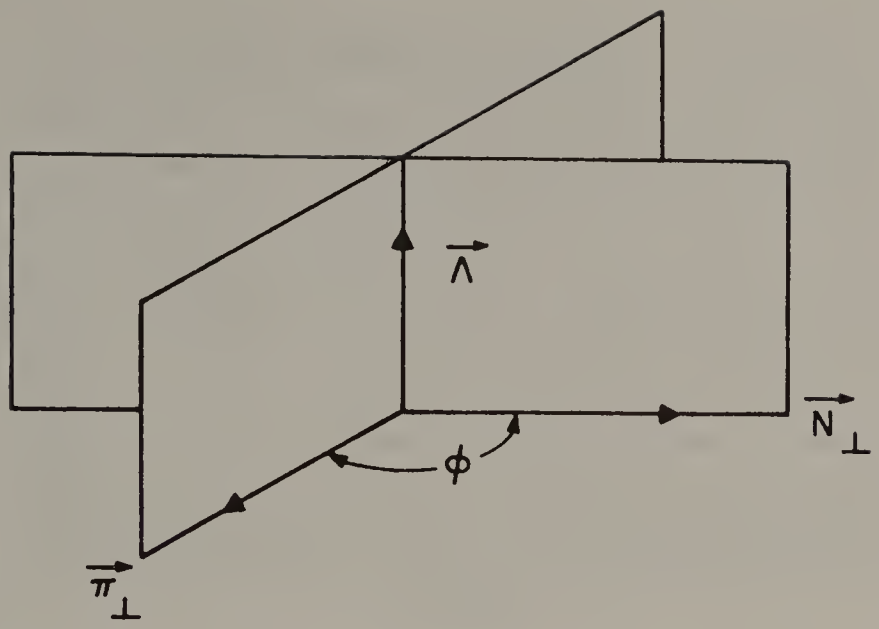

Figure 2. The dihedral angle ϕ between the production plane and the decay plane. By definition, ϕ varies from 0 to π. However, if one views ϕ also as the angle of rotation around $\vec{\Lambda}$, its range can be increased from 0 to 2π.

I was quite excited, and urged Steinberger to re-analyze his data immediately and test the idea experimentally. This led to the very first experimental attempt to detect parity nonconservation. Because it was relatively simple to re-plot their data in terms of the new ϕ, from 0 to 2π, very soon, Steinberger and his collaborators (Budde, Chrétien, Leitner, Samios and Schwartz) had their results,[6] which were published even before the theoretical paper[7] on parity nonconservation. The odds turned out to be 7 to 15 in Λ° decay and 13 to 3 in Σ^- decay (see Figure 3). Of course, because of the limited statistics, no conclusion could be drawn. Nevertheless, these preliminary data were very encouraging. The implication was immense and I felt elated by the breakthrough. Parity nonconservation obviously had many other and further consequences, which I began to probe intensively. By the end of April, I had essentially completed the theoretical investigation of strange particle decays.

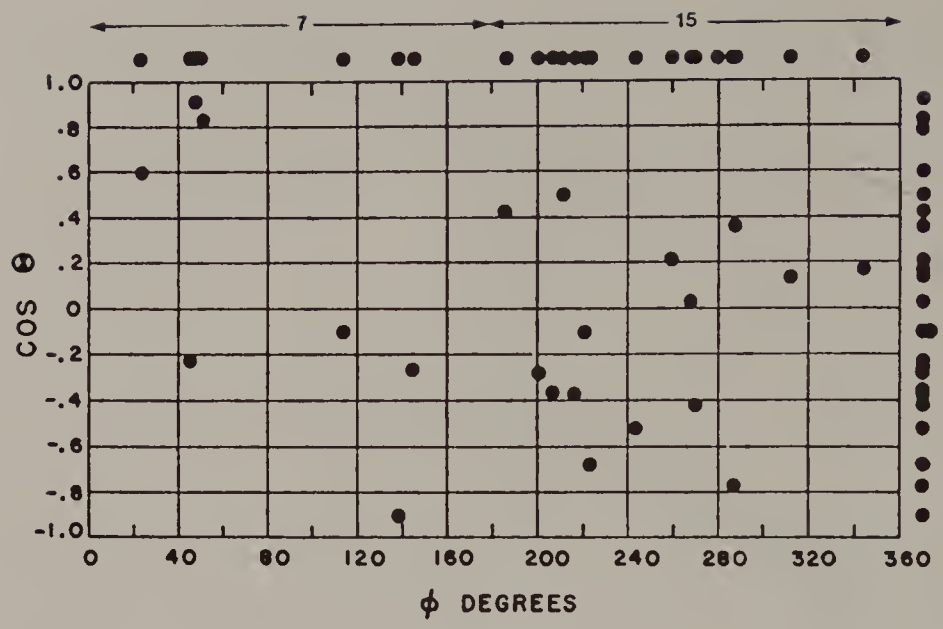

Figure 3a. Angular correlation plot of the cosine of the polar angle against the azimuthal angle ϕ for the reaction $\pi^- + p \rightarrow \Lambda^o + \theta^o$, $\Lambda^o \rightarrow \pi^- + p$. The point histograms on the edges of the figure represent the data after integration over the other coordinate. (Taken from Ref. 6.)

During that period, I had several discussions with Steinberger. One day around the beginning of May he came to see me and said that he had just given a seminar at Brookhaven on his experiment. Yang had been in the audience and had strongly objected to my idea that there could be asymmetry in Steinberger's hyperon experiment due to parity violation. I then called Yang and told him that since we last saw each other, at the Rochester Conference, there had been a theoretical breakthrough. I asked him not to express his objections further in public until he could talk it over with me.

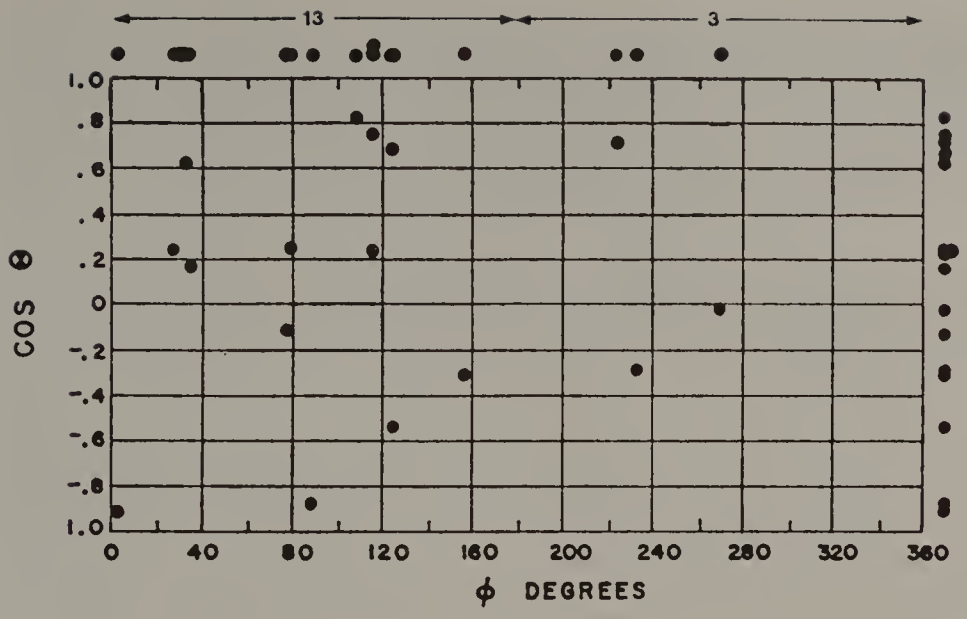

Figure 3b. Angular correlation plot for the reaction $\pi^- + p \rightarrow \Sigma^- + K^+$, $\Sigma^- \rightarrow \pi^+ + n$. (Taken from Ref. 6.)

The next morning Yang drove from Brookhaven to Columbia and we had a very intensive session. I told him my idea and analysis in detail. He was quickly convinced of its importance and wanted to work on it with me.

Yang is endowed with a highly critical mind. It always made me feel more confident if I could overcome his objections with substantive arguments. Furthermore, he is an excellent physicist. The implications of parity nonconservation extended over all facets of physics. I thought Yang's participation would undoubtedly enrich the final product. Therefore I expressed my welcome.

On that day, we covered an enormous variety of physical processes, arguing, debating, sometimes even shouting; occasionally we would switch our points of view, just to make sure. The tremendous intensity of our working together in opposition and in harmony, the immense open feeling that the whole world was lying in front of us, the fearlessness of youth,these are what made life meaningful!

Both of us, of course, knew there existed very good experimental evidence for parity conservation in the strong reaction. But how solid was it? By the end of the day, we were convinced that for strong interactions it was indeed very good (even for strange particles). In addition, there was also excellent proof for parity conservation in the electromagnetic interaction.

My original idea, that all weak decays of strange particles could be parity violating, stood firm. However, for the weak interaction of non-strange particles further investigation was needed, and both of us agreed we should look into that.

Before Yang joined me on that day, I had been thinking of writing up the work I had already done on parity nonconservation in strange particle decays. However, Yang convinced me to hold off on this since it was a better idea to enlarge the scope to encompass the whole weak interaction. We separated and each of us continued the examination of parity questions in beta decay.

Beta decay is a field with a long history. A very large body of knowledge was available at that time. Since parity conservation had been an implicit assumption in all analyses, and the notion of parity was commonly used, it required a very careful examination of all the available experimental facts to see whether parity nonconservation could also be extended to beta decay. One of the world's great experts on beta decay is C.S. Wu, whose office was a few floors above mine at Columbia. I visited her and told her of these ideas. She was extremely interested in them and kindly lent me the authoritative book on beta decay edited by K. Siegbahn.[8]

At that time, because of parity conservation, beta decay was described by an interaction consisting of five coupling constants, C_i (i = S, P, V, A, T). For our purpose, we introduced five additional parity-violating constants C_i'. Yang and I started a systematic investigation of all known beta decay phenomena by using the general parity nonconserving interaction. We raced through the Siegbahn book and kept in touch often by telephone. It took us about two weeks to finish the entire beta-decay analysis. (In computational skill, Yang and I were evenly matched. This race was extremely intense, but in the best spirit of collaboration and competition, with both of us as winners. Any attempt to try to place which one was ahead in such intensive and extensive calculations is meaningless.)

We began with the allowed spectrum and we found, after some calculation, exactly the same β-distribution with a simple replacement of the usual

$$C_i^* C_j \quad \text{by} \quad C_i^* C_j + C_i'^* C_j' \ . \tag{7}$$

Next came the forbidden spectrum, and the same thing happened. We then computed the Coulomb effect, the $\beta-\nu$ correlation, the $\beta-\gamma$ correlations and the (then-existing) β and γ results from polarized nuclei; while some of these calculations were rather complicated, in the end all interference terms between C_i and C_j' invariably were cancelled, and the same old expressions appeared, provided we made the same simple replacement given by (7).

Finally we reached the end of the Siegbahn book, rederiving all the old formulas with this new interaction. It then became clear that there was not a single evidence for parity conservation in beta decay. We got together to compare notes. As we talked we realized that we must have been very stupid, and that there had to be an extremely simple reason why all those complicated interference terms C_i*C_j' cancelled each other. Once we stopped calculating and started to think, within a very short time it was clear that the reason for this lack of evidence was the simple fact that nobody had made any attempt to observe a physical pseudoscalar from an otherwise seemingly right-left symmetrical arrangement!

From then on, the theoretical analysis could be made independently of any detailed calculations. We then expanded our scope to other processes, such as $\pi-\mu$ decay. It took another month of intensive work for us to complete the analysis and to write the theoretical paper.[7] The next scene of the parity drama was set, with many more participants.

There remained the experimental question of how to detect possible parity violation in beta decay. Again I discussed with C.S. Wu the best way to measure asymmetry in spin-momentum correlation in beta decay. She suggested the possible use of a strongly polarized ^{60}Co β-source, for which however a low-temperature set-up was needed. Subsequently, she approached the Bureau of Standards in Washington. This led to the decisive experiment by Wu, Ambler, Hayward, Hoppes and Hudson,[9] which was followed within a few days by the detection of parity violation in $\pi-\mu$ decay, by Garwin, Lederman and Weinrich,[10] and by Friedman and Telegdi.[11] Because of the practical difficulty in accumulating data on strange particles, parity violation in Λ- decay was established half a year later by the Steinberger group.[12]

Reflections on a Collaboration

In recounting these happy days of thirty and forty years ago, I re-read the account[13] given by Yang in 1983. Once again, I was distressed and amazed by what he wrote.

For instance, take his version of the breakthrough on parity violation. According to Yang, one day in late April or early May of 1956 he came from Brookhaven to visit me at Columbia, and during our conversation in a restaurant he suddenly had the idea of detecting parity nonconservation in Λ-decay. He further said that when he told that idea to me, I resisted.

It is a matter of record that, weeks before Yang visited me on that day, I already had the idea. I explained it to Jack Steinberger shortly after the Rochester Conference which took place in early April. In fact, the first experimental attempt to detect parity violation in β-decay had already been carried out at Columbia by Budde, Chrétien, Leitner, Samios, Schwartz and Steinberger before Yang joined me on that day. Their result was published in Physical Review[6] (with an acknowledgment to me), before the theoretical paper[7] by Yang and myself.

In his version of our collaboration, Yang has made a systematic effort to paint a picture of himself as leader, from beginning to end, with me as subordinate. Any physicist who knew us in those days would know this to be untrue. Our collaboration had always been one of equal partnership. Our talents were different, but complementary. That is why the collaboration was successful.

Yang's effort to present such an image extends even to our early days in Chicago. According to his account, although I was Fermi's student, Yang was in effect my teacher. This is indeed extremely strange, considering that Fermi was one of the great teachers of all time. At Chicago, Yang and I were both graduate students. We had many discussions, with lots of give and take, and we enlarged each other's horizons. But how could Yang ever mistake these exchanges between novices for the quality of guidance and instruction that I received from Fermi?

From May 1948 to December 1949, Fermi directed my research over a wide range of subjects, which included, besides particle and nuclear physics, heat conductivity of dense matter, white dwarfs, stellar stability, hydrodynamics and magneto-hydrodynamics. These led to several papers which, except for the Lee-Rosenbluth-Yang article, had nothing to do with Yang.

The collaboration between Yang and myself ended about twenty-five years ago. Its merit, as represented by our joint papers, has withstood the test of time. Both of us have made many other independent contributions to physics. The world has rewarded us with recognition and success. Shouldn't that be sufficient?

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C. K. Jen and X. D. Xie

T. D. Lee and T.-Y. Wu

PART 3

- The CUSPEA Sessions -

November 23, 1986

A Gathering of Chinese Physicists at Luncheon

A BRIEF INTRODUCTION TO CUSPEA

The China-U.S. Physics Examination and Application program (CUSPEA) was established in 1980 by T. D. Lee to enable highly qualified Chinese physics students to study for the Ph.D. at American universities. A physics examination, on the level of U.S. graduate schools' first-year qualifying exams, is made up each year in the U.S. and is taken by the students together with a written English test composed by Academia Sinica in Beijing. The exams are held in a number of cities in China, and are administered by the Chinese State Council of Education and Academia Sinica. A team of more than sixty Chinese professors of physics volunteer annually to do the grading.

Each year more than 650 students take the exam, from which about 100 of the top-ranking are selected; they are then interviewed by the two U.S. professors who wrote the physics examination, and also by their wives. The interviewers discuss the students' interests and preparation with them individually and also grade their spoken English. This interview information and the exam scores are incorporated into a student record booklet which is then distributed to the approximately 90 American member schools for their use in evaluating their CUSPEA applicants. The students, many of whom receive several offers, then come to the United States the following fall and take up their studies as regular teaching or research assistants.

This is now the eighth year of CUSPEA; nearly 800 students have come to the U.S. Almost all of them are at the top of their classes in graduate school. Some of the earlier ones have now completed their studies and are doing postdoctoral work either in China or in the U.S. The four talks given at the CUSPEA part of the symposium are representative of the diversity of their specialties.

The success of CUSPEA is due largely to the strong support of scholars from both nations. Special acknowledgment must be given to Ny Tsi-ze and Wu Tang of Academia Sinica, and to Shen Keqi and Zhao Kaihua of Peking University. On the U.S. side, we are particularly grateful to the interviewers:

Norman and Winifred Christ	(1980)
Douglas and Janet Fitchen	(1980, 1981)
Sam and Joan Treiman	(1981, 1982)
Jack and Letha Sandweiss	(1982, 1983)
George and Maya Trilling	(1983, 1984)
Malvin and Paula Ruderman	(1984, 1985)
Larry and Grace Spruch	(1985, 1986)
Norman and Sally Kroll	(1986).

Y. Pang, E. Henley, D. Fitchen

T.-Y. Wu, T. D. Lee, T.-Z. Ny, K. C. Chou

FERMION PROPAGATOR AND COMPOSITE BOSON PROPAGATORS ON RANDOM LATTICES

Yang Pang

Columbia University

Random lattice field theory proposed by Christ, Friedberg and Lee is a promising candidate for the fundamental theory of space and time[1]. It also provides an alternative approximation to study the various non-perturbative aspects of continuum field theory. One fundamental constant, lattice spacing l, is introduced in this theory. On a distance scale much larger than l, one requires the theory to give the same result as that of a continuum theory. The formulation of fermion fields on regular lattices exhibits the problem of species doubling, which makes the theory different from the continuum theory for distances much larger than l[2]. Here I shall report some recent results Dr. H.C. Ren and I had on the random lattice fermion fields[3,4].

Let us start with one-dimensional case. The fermionic action in the one dimensional continuum is

$$A_C = \int dx\ (\bar{\psi}\gamma_1 d\psi/dx + m\bar{\psi}\psi), \tag{1}$$

where $\bar{\psi}$ and ψ are two independent two-component Grassmann spinors and we choose

$$\gamma_1 = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}, \qquad \gamma_5 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}. \tag{2}$$

The continuum fermion propagator is given by

$$S^C_F \equiv <\psi(x)\bar{\psi}(0)> = \theta(\gamma_1 x)exp(-m|x|), \tag{3}$$

and the composite boson propagators are defined to be

$$\Delta^C(x) \equiv -\langle\psi(0)\bar{\psi}(0)\psi(x)\bar{\psi}(x)\rangle_{conn} = 0, \tag{4a}$$

$$\Delta^C{}_5(x) \equiv -\langle\psi(0)\gamma_5\bar{\psi}(0)\psi(x)\gamma_5\bar{\psi}(x)\rangle_{conn} = exp(-2m|x|), \tag{4b}$$

where $\langle\cdots\rangle_{conn}$ is the connected part of the expectation value $\langle\cdots\rangle$ and $\theta(x)$ is the step function, zero for $x<0$ and one for $x>0$.

We can write the lattice action by assigning each lattice site i, at x_i, two independent two-component Grassmann spinors, $\bar{\psi}_i$ and ψ_i, and replacing the derivative in *(1)* by a difference. We then have

$$A_L = \tfrac{1}{2}\sum_{i=1}^{\mathcal{N}}\bar{\psi}_i\gamma_1(\psi_{i+1}-\psi_{i-1}) + m\sum_{i=1}^{\mathcal{N}}\omega_i\bar{\psi}_i\psi_i = \bar{\psi}\mathcal{A}\psi, \tag{5}$$

where $\omega_i = (x_{i+1}-x_{i-1})/2$, $\mathcal{N}$ is the total number of sites, and $0\leq x_i<\mathcal{L}$. For regular lattices $x_{i+1}-x_i = \mathcal{L}/\mathcal{N} = l$, and for random lattices x_i's are arbitrary. To guarantee the Hermiticity of the lattice action, the difference in *(5)* is not taken between the nearest neighbors but between the next nearest neighbors.

The discrete version of the Dirac equation can be obtained by the variation of *(5)*,

$$\tfrac{1}{2}\gamma_1(\psi_{i+1}-\psi_{i-1}) + m\omega_i\psi_i = 0. \tag{6}$$

Because the difference is between the next nearest neighbors this equation always has two degenerate solutions. If $\{\psi_i\}$ is one of the solutions, then $\{(-)^i\psi_{\mathcal{N}-i+1}\}$ is also a solution with the same energy.This is called species doubling. For regular lattices, the result can be generalized to higher dimensions, in general there will be 2^D fermion species in D dimension. Because of the species doubling, physical quantities calculated from the regular lattice action will in general differ from the same quantities calculated from the continuum action even on a distance scale much larger than l. For random lattices as we shall see, the extra species do not contribute when the distance is much larger than the average lattice spacing l.

For a given random lattice, the lattice propagator from site a to site

b is

$$(S^L{}_F)_{ab} = \frac{\int \Pi d\psi_i d\psi_i exp(-A_L)\psi_a\psi_b}{\int \Pi d\psi_i d\psi_i exp(-A_L)} = (\mathcal{A}^{-1})_{ab} \qquad (7)$$

$$= \begin{pmatrix} f_{ab} & 0 \\ 0 & f_{ba} \end{pmatrix}.$$

To get a translational invariant quantity, it is necessary to perform an average on an ensemble of random lattices with fixed site a and site b, which gives us the space–time propagator

$$S^L{}_F(x) = \frac{\int \Pi dx_i \mathcal{J}(x_1,\cdots,x_N)(S^L{}_F)_{ba}\delta(x_a)\delta(x_b-x)}{\int \Pi dx_i \mathcal{J}(x_1,\cdots,x_N)\delta(x_a)\delta(x_b-x)} , \qquad (8)$$

where $\mathcal{J}$ is an appropriate jacobian. It can be shown that[5]

$$f_{ab} \sim \begin{cases} exp(-m|x_a-x_b|) & x_a-x_b > 0, \\ (-)^{b-a}exp(-m|x_a-x_b|) & x_a-x_b < 0. \end{cases} \qquad (9)$$

After the ensemble average, $f_{ab}\to f(x)=\theta(x)exp(-m|x|)$, so that the space time propagator agrees with the continuum propagator *(3)*.

One can also calculate the composite boson propagator as the ensemble average of the corresponding lattice propagator. Define

$$(\Delta^L)_{ab} \equiv trace(\mathcal{A}^{-1}{}_{ba}\mathcal{A}^{-1}{}_{ab}) = 2f_{ab}f_{ba} , \qquad (10)$$

$$(\Delta^L{}_5)_{ab} \equiv trace(\gamma_5\mathcal{A}^{-1}{}_{ba}\gamma_5\mathcal{A}^{-1}{}_{ab}) = f_{ab}{}^2+f_{ba}{}^2 . \qquad (11)$$

For $|x_a-x_b|=|x|>>l$, the ensemble average of *(10)* will give us *(4a)*, but the ensemble average of *(11)* is very different from *(4b)*,because $f_{ab}{}^2\neq f^2(x)$. This problem can be solved if one uses the point splitting technique. Instead of *(11)*, let us define

$$(\Delta^L{}_5)_{aa';bb'} \equiv trace(\gamma_5\mathcal{A}^{-1}{}_{ba}\gamma_5\mathcal{A}^{-1}{}_{a'b'}) = f_{ab}f_{a'b'} + f_{ba}f_{b'a'} . \qquad (12)$$

The ensemble average is then carried out by fixing points a,a',b,b' with $x_a-x_b=x_{a'}-x_{b'}=x>>l$ and $x_{a'}-x_a=x_{b'}-x_b=\epsilon\sim l$. From *(9)* we have $f_{ab}f_{a'b'}\to f^2(x)$ and $f_{ba}f_{b'a'}\to 0$. So the ensemble average of *(12)* is *(4b)*. The numerical results are shown in *Fig.1*.

We did similar calculations in higher dimensions. Correct continuum

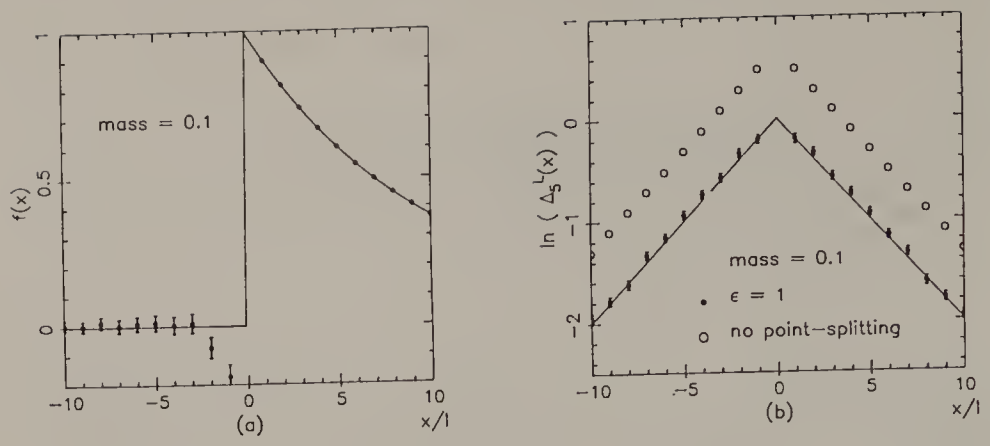

Fig.1.Ensemble average over 500 one-dimensional random lattices of 100 sites with fermion mass $m=0.1$ and with the periodic boundary condition. (a) Fermion propagator (b) $\Delta^L_5(x)$ with and without point splitting.

limits for the fermion propagator are obtained by the ensemble averages and with the use of point splitting technique the composite boson propagators also approach correct continuum limits within *3~4* lattice spacings.Two-dimensional results are shown in *Fig.2*,and four-dimensional results are shown in *Fig.3*.

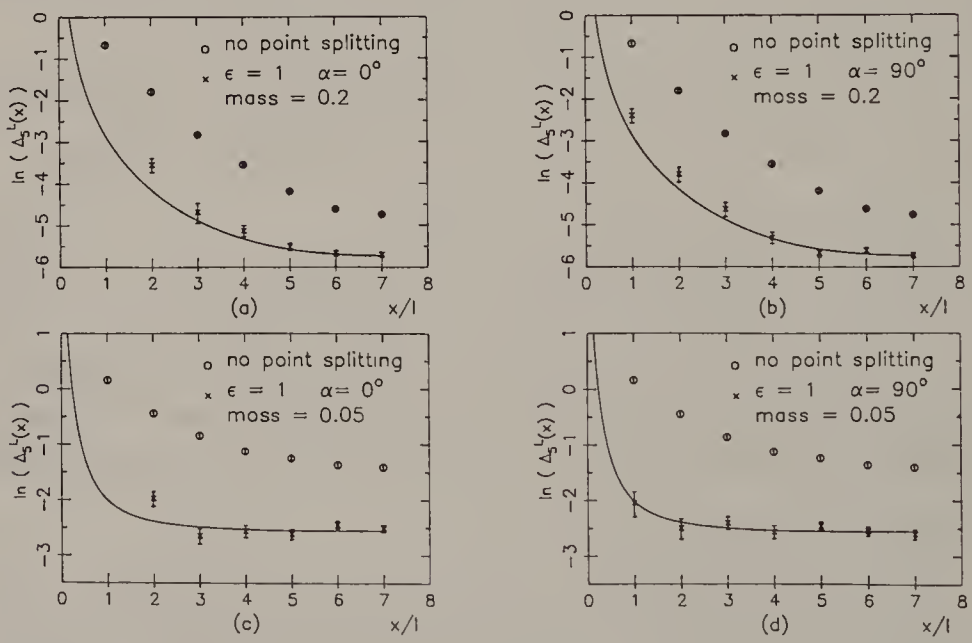

Fig.2.Pseudo-scalar propagator in two dimensionscalculated with and without point splitting for the periodic boundary condition. For propagators without point splitting (represented by circles) we average 1000 random lattices. For propagators with point splitting (represented by crosses) we average over 500 random lattices. α is the angle between vectors $x_{a'}-x_a$ and x_b-x_a.

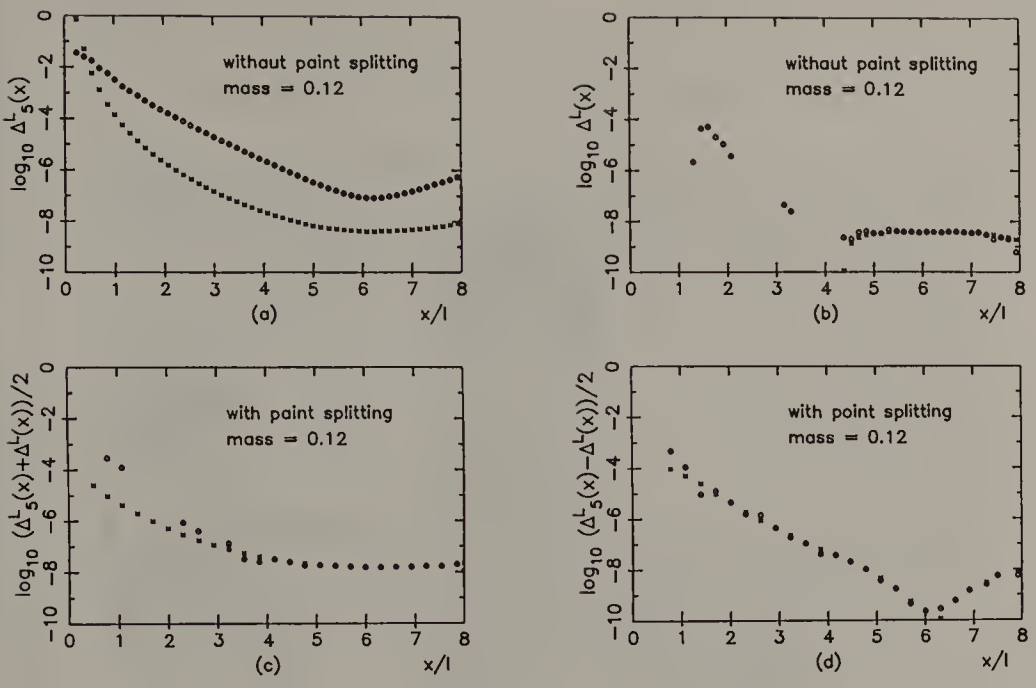

Fig.3. Composite boson propagators in a four-dimensional random lattice. Crosses represent the correct continuum limits while the circles represent the averages of the lattice propagators. (a) and (b) without point splitting, (c) and (d) with point splitting.

Acknowledgement

Most of this work was carried out with the collaboration of Dr.H.C. Ren. We would like to express our gratitude to Professor T.D.Lee for his suggestion of this problem and for his encouragement.

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N. Qi

RESONANT PHOTO-EXCITATION LASERS IN Be-LIKE CIII IONS

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ABSTRACT - Intense, MnVI line radiation at 31.0nm from a laser produced plasma was used to resonantly pump CIII ions, in an adjacent vacuum-arc discharge, from the 2s2s ground state to the 2s4p upper level. Electron collisions rapidly redistribute the population of the pumped 4p level among other n=4 levels. Enhanced fluorescence coincident with the MnVI line radiation was observed on many 4-3 transitions in CIII. To describe the kinetics in CIII ions, a 72-level collisional radiative model was constructed with most up-to-date atomic data. Measured values of electron density and temperature in the C and Mn plasmas were used as inputs to the model. Small-signal gain coefficients of about 0.2/cm were predicted by the model.These gains were found to be consistent with the experiments. Single-pass gain measurements showed that the total gain-length product was 0.8 in CIII lines at 217.7 and 216.3nm. Laser oscillation at these wavelengths was demonstrated in a resonant Fabry-Perot cavity. This CIII-MnVI photoexcitation scheme is a prototype for soft X-ray lasers in higher Z, iso electronic analogs.

I. INTRODUCTION

Much effort has been concentrated on finding suitable processes for producing X-ray lasers. Recently, soft X-ray amplifiers have been demonstrated[1,2] in laser produced plasmas pumped by electron collisions and by recombination. Resonant photoexcitation of one ion by intense line radiation from another ion, is another approach to soft X-ray lasers. The main difficulty arising in resonant photoexcitation lasers is to find resonances between the pump and pumped transitions. These resonances must be within the line widths of the corresponding transitions. As early as 1930, enhanced fluorescence at 3.2μ in CsI was observed[3], using 388nm pump line radiation from HeI. In 1961, such resonant photoexcitation led to laser oscillation[4] at 3.2μ. Using photoexcitation of He atoms and then collisional transfer to the other high-lying levels of neon, laser oscillation at 1.8, 1.6 and 1.3μ in NeI was reported[5]. The concept was extended [6] to 546.1nm in HgI, using two step photoexcitation. In 1984, Krishnan and Trebes [7] proposed a new class of photoexcitation lasers in Be-like ions with lasing wavelengths from 217.7nm in CIII pumped by MnVI line radiation to 23.0nm in MgIX pumped by AlXI. This paper describes theoretical studies (in section II) and experimental studies (in section III) of the prototype CIII-MnVI scheme. Since CIII is isoelectronic with MgIX, the collisional radiative kinetics elucidated by this research shed light on possible soft X-ray lasers in MgIX.

II. THE KINETICS OF THE CIII-MnVI SCHEME

Figure 1 illustrates the principle of resonant photoexcitation. MnVI line radiation at 31.0182nm from a laser produced plasma pumps the CIII ions from the 2s2s ground state to the 2s4p level. The 0.0012nm

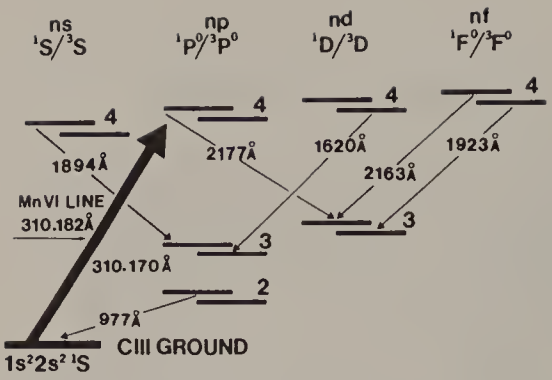

Fig.1. Energy levels in CIII. Photoexcitation of the 2s-4p transition is accompanied by collisional transfer to the 4s, 4d and 4f levels. Lasing is possible on 4s-3p, 4p-3d and 4f-3d transitions.

wavelength mismatch between the pump and pumped lines is overcome by the Doppler width of the MnVI pump line. For intense MnVI line radiation, population inversion can occur between 4p and 3d, and a laser is possible at 217.7nm. Due to electron collisions in the CIII plasma, the population of the pumped 4p level is rapidly transferred to other n=4 levels. Lasing is also possible on other 4-3 transitions, as shown in the figure. In general, plasma conditions required for the pump and the pumped media are different. In the pump Mn plasma most ions should be in the MnVI charge state,while in the pumped medium most ions should be in the CIII ground state. Such disparate requirements can be accomplished by keeping the two plasmas physically separated, and thus minimizing the hydrodynamic interaction between them. To determine what plasma conditions are required to achieve laser oscillation, the population densities of various excited states in CIII must be known. The change in these populations upon photoexcitation then allows the gain to be estimated. To this end, a computer model for the populations of CIII excited states was developed.

The calculation of the excited state populations came out in two steps. First, a modified coronal ionization balance was used to calculate the population of each carbon ion charge state in the ground level. Then,using the calculated populations of the CIII and CIV ground states and with electron density and temperature as inputs, the excited states in CIII were calculated by a collisional radiative model. Such a two-step approach is valid when the cumulative population of all the excited states is a small fraction of that of the ground state. This is indeed the case for the densities and temperatures of relevance to this work. We obtained very detailed atomic data for CII, CIII and CIV ions from Lawrence Livermore National Labs., provided by Dr. W.L.Morgan. Using these data a 72-level, collisional-radiative model [8] for the excited states in CIII was constructed. In this model, all singlet and triplet states of the outer electron configurations with principal quantum numbers n=2 to 6 were included. For n=7 to 10, single lumped states distinguished only by principal quantum number were included. The collisional and radiative processes considered were electron collisional ionization, electron collisional excitation and de-excitation, radiative and 3-body recombination, and spontaneous emission. Dielectronic recombination was ignored compared with these processes. Selective photo-excitation was accounted for in the model by including stimulated emission and absorption of the 4p-2s transition due to the MnVI 31nm line radiation. In the experiment, the estimated[8] brightness temperature of the MnVI pump line was 20eV. Using this pump line intensity, gains of 4-3 transitions in CIII were calculated. Figure 2(a) shows predicted gains for the 4s-3p, 189.4nm, 4f-3d, 216.3nm, and 4p-3d, 217.7nm lines. Including the effect of trapping for all the transitions which terminate on the n=2 levels, it is found that the gain of the 4f-3d, 216.3nm line decreases more

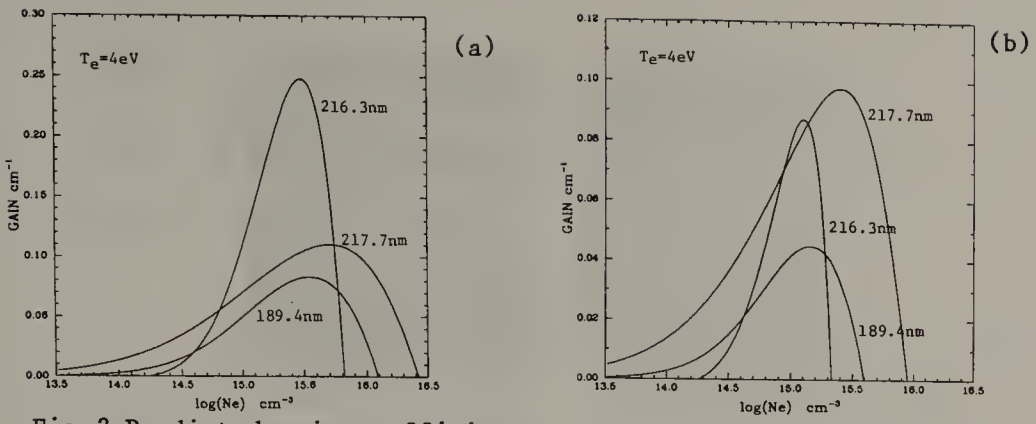

Fig. 2 Predicted gain coefficients vs Ne for the 217.7, 216.3 and 189.4nm lines in CIII (Te=4eV). (a) Without optical trapping. (b) With optical trapping.

rapidly than that of others, as shown in Fig. 2(b). This is reduction due to the shorter lifetime of the 4f level. At lower electron densities, the gain is lower, since fewer electrons are available to be pumped from the ground state to the upper 4p level. At higher electron densities, electron collisions destroy the population inversion, by collisional mixing of the n=4 and 3 levels, or by excitation out of the n=4 levels. The predicted peak gain of about 0.2/cm at 217.7 and 216.3nm in CIII occurs at $N_e=5\times10^{15}cm^{-3}$ and T_e=4eV. Such conditions for the pumped plasma were produced in a vacuum discharge.

III. THE EXPERIMENTS AND RESULTS

Figure 3 is a schematic drawing of the experimental apparatus. The CIII ions are produced in a vacuum-arc discharge between a 6mm diameter carbon cathode and a hollow carbon anode. The separation of the electrodes is 100mm. The carbon cathode is located 5mm off the optical axis of a spectrometer, which was used to measure fluorescence and gain due to photo-excitation. The discharge was triggered by focusing a CO_2 laser onto the carbon cathode. A typical discharge current waveform is shown in Fig.4(a). The rise-time is 15μs, the flat-topped width about 50μs and the peak current is 6.4kA. The discharge plasma so produced consists of mostly CIII ions, with an electron density $5\times10^{15}cm^{-3}$ and temperature 4eV as measured[8]. The pump Mn plasma was produced by focusing another 15J/150MW CO_2 laser to a 2mm x 23mm line focus on a Mn slab, as shown in Fig. 3. This Mn plasma is 7mm distant from the spectrometer axis and begins 5mm from the cathode surface.

Typical enhanced fluorescence, coincident with the MnVI line radiation, is shown in Fig.4. Fig.4(b) shows the spontaneous emission at 217.7nm, in the absence of the Mn pump plasma. The trace shown is an average over five successive discharges. Figure 4(c) shows the enhanced fluorescence at 217.7nm, when the laser produced Mn plasma was created, 36μs after discharge initiation. To capture this signal, the sensitivity of the detecting electronics was decreased by a factor of 45. To be sure that the enhanced fluorescence came from the CIII ions, the Mn plasma was created without the C discharge. In this case, Fig.4(d) shows that there was some small level of spurious background radiation at 217.7nm from the Mn plasma. When this background was subtracted from the fluorescence in Fig.4(c), the net enhanced fluorescence was 180 times the spontaneous emssion. Such enhancements were found to be very reproducible from shot to shot. The typical duration of the enhanced fluorescence (FWHM) was 0.5-1.0μs. Similar fluorescence was observed on several other n=4-3 transitions[9]. The

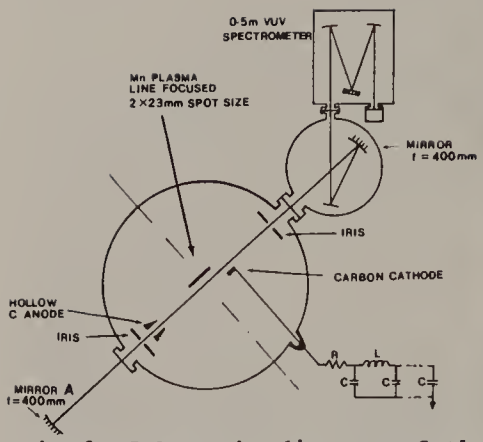

Fig. 3. Schematic diagram of the experimental apparatus. Mirror A was used for the single-pass gain measurements.

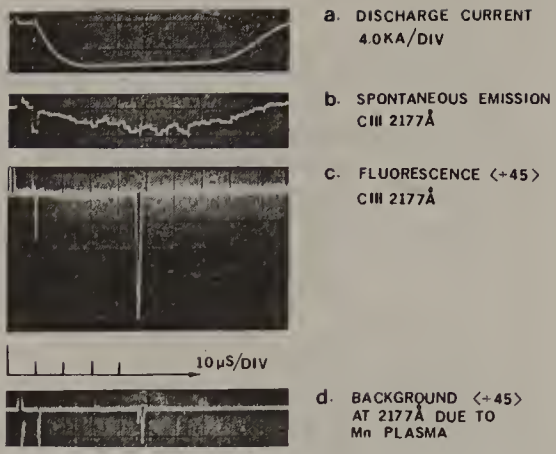

Fig. 4. Typical observed fluorescence at 217.7nm in CIII.

measured ratios of enhanced fluorescence to spontaneous emission were 90 and 70 at the 4s-3p, 189.4nm and 4f-3d, 216.3nm lines, respectively. These measured ratios (180, 90 and 70) are very close to those predicted by the CR model[8], i.e. 140, 90 and 90. It might be argued that the observed fluorescence is due to electron collisional excitation in CIII by hot electrons from the laser produced Mn plasma. If this were the case, a laser produced Al plasma should also produce such fluorescence. When an Al plasma was produced at the same location as the Mn plasma, no fluorescence was observed. Thus, selective photo-excitation was confirmed. Population inversions could ocuur at these n=4-3 transitions. To verify this, gain measurements were undertaken.

To measure gain, a mirror A was added to the spectrometer axis, as shown in Fig. 3. The mirror was adjusted to reflect rays from the mid-region of the carbon plasma back through the plasma. Irises were placed on either side of the discharge, to insure that the solid angles subtended by the mirror and the spectrometer gathering optics were identical. On successive shots, the line emission was measured with and without mirror A. The ratio R of the measured line intensities with and without reflecting mirror A is given by:

$$R = 1 + s \exp(G) \qquad (1)$$

where s is the net reflectivity of the vacuum window and mirror A, and G is the integrated gain coefficient along the entire optical path length. For non-inverted and optically thin lines, G = 0, hence R = (1+s). For an inverted transition with gain, G>0 and R>(1+s). The line chosen for the calibration of the net reflectivity s was a CII line, at 217.4nm. This line was measured to be optically thin and is very close in wavelength to the CIII, 217.7 and 216.3nm lines on which gain was measured. The measured value of s was 0.5. Also, the ratio R was measured[10] for the spontaneous emission on the CIII line at 216.3nm. Again, s was found to be 0.5. Figure 5 shows the measurements of ratio R at 217.7 and 216.3nm in CIII with the MnVI pump line radiation, and evidence for gain. Figure 5(a) shows the enhanced fluorescence at 217.7nm, without reflecting mirror A. The slight baseline shift is due to a ground-loop in the discharge circuit. Figure 5(b) shows the fluorescence with mirror A. The ratio of R was 2.1. For s=0.5, this gives G=0.8. For a 2cm effective gain length in the CIII plasma, the measured gain coefficient was 0.4/cm. Figures 5(c) and 5(d) show similar results on the CIII line at 216.3nm wavelength. For this line too, R was found to be 2.1 and the gain coefficient was 0.4/cm. The measured gain coefficients are higher than

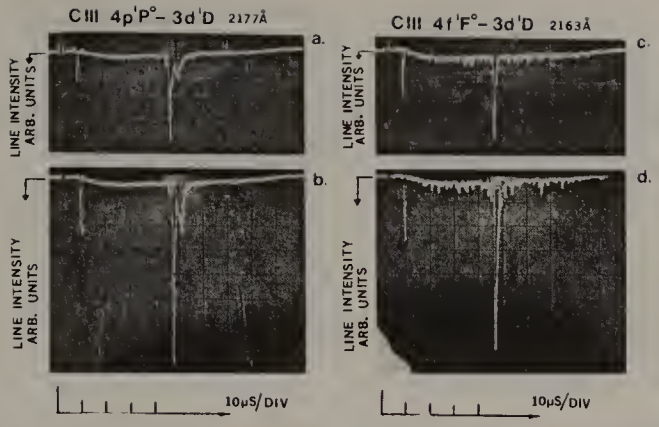

Fig.5 Single-pass gain at 217.7 and 216.3nm in CIII.
(a) Enhanced fluorescence at 217.7nm, without mirror A;
(b) Fluorescence at 217.7nm, with reflecting mirror A;
(c) Fluorescence at 216.3nm without reflecting mirror A;
(d) Fluorescence at 216.3nm with reflecting mirror A.

those predicted by the collisional radiative model. The discrepancy can be due to the uncertainty in transition probabilities and the brightness of the MnVI pump line radiation.

Laser oscillation at the 217.7 and 216.3nm wavelengths was demonstrated in a Fabry-Perot cavity. This confocal resonator consists of two mirrors, which have a 1m radius of curvature and a slightly less than 2m separation to insure the cavity stability. Two Brewster windows were used to seal the vacuum. The cavity was aligned using an external He-Ne laser. For a perfectly aligned cavity, it is straightforward to derive an expression for the ratio of the measured line intensity with the front cavity mirror (near the spectrometer) removed, I, to that with the tuned cavity, I′, viz:

$$\frac{I}{I'} = \frac{1 - \exp(2G-2L)}{(1-r)\{1 - \exp[(N+1)(G-L)]\}} \qquad (2)$$

where G is the gain coefficient, N is the number of passes, L is the cavity loss and r(=0.94) is the reflectivity of the cavity mirror. For a non-inverted line (G=0), this ratio is greater than one. When G>0, lasing requires that the gain should be greater than the cavity losses. Then the ratio is less than one. For lasing lines, removing the front mirror will actually cause a decrease in the detected signal, rather than an increase. Such a dramatic difference in the ratio, from greater than unity to less than unity, is usually a reliable measure of lasing on any line. Ratios R were measured at 217.7 and 216.3nm, as shown in Fig.6. Figure 6(a) shows the measured emission at 217.7nm, with the tuned laser cavity. On this scale, no spontaneous emission is observed before and after photoexcitation, and strong emission occurs coincident with the MnVI pump radiation. When the front mirror was removed, Fig.6(b) shows that the detected emission decreased. Such a decrease confirmed laser oscillation at 217.7nm. Similar results were obtained at 216.3nm, on the 4f-3d transition. Figure 6(c) shows the laser intensity with the tuned cavity. Figure 6(d) shows a decrease in intensity, when the front mirror was removed. Again, this decrease comfirms laser oscillation at 216.3nm. By contrast, when this experiment was repeated at 229.7nm in CIII, the detected signal increased, by a factor of 4:1, when the front mirror was removed. To ensure that opacity effects played no role, the experiments were performed at the far wing of this CIII line, where the plasma was optically thin.

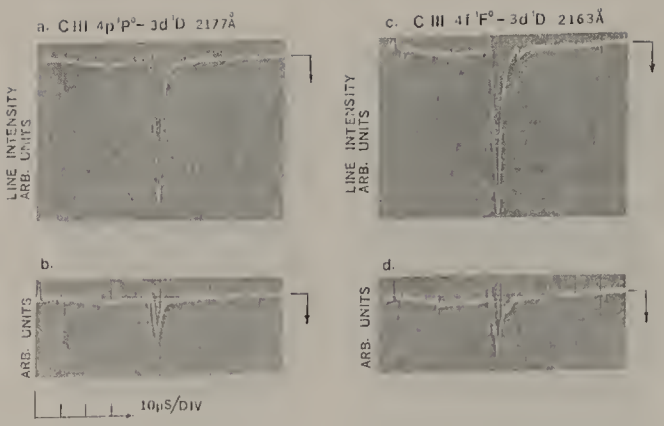

Fig.6. Evidence for lasing in CIII. (a) Emission at 217.7nm, with laser cavity; (b) Emission at 217.7nm with the front mirror removed; (c) Emission at 216.3nm, with the laser cavity; (d) Emission at 216.3nm with the front mirror removed.

IV SUMMARY

In summary, a model for the kinetics of the CIII-MnVI laser scheme has been described. Electron density and temperature in the C and the Mn plasma were measured. Enhanced fluorescence at 4-3 transitions in CIII due to MnVI line radiation was observed. Single-pass gain of 0.4/cm was measured at 217.7 and 216.3nm in CIII. Laser oscillation at these wavelengths was demonstrated. It should be pointed out that the experimental configuration used is far from optimal. By making the pump plasma coaxial with the laser medium, larger solid angles are possible for photoexcitation and thus increasing the gain. Based on the model of the CIII-MnVI scheme, similar calculations for the next higher z scheme, the NIV-PIX scheme, were carried out. Predicted gain of 1.0/cm on the 4p-3d transition at 128.6nm in NIV occurs at N_e = 5x $10^{16}cm^{-3}$ and T_e = 8eV. It is possible that soft x-ray laser analogs of the CIII-MnVI laser discussed here could be also produced.

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NUCLEAR SHADOWING AND PERTURBATIVE QCD

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It was found[1] some years ago that the cross section for virtual photons measured in deeply inelastic scattering off nuclei is not equal to A times the photon nucleon cross section. In the energy regime $Q^2 < 2\sim3\ GeV^2$, the experimental data show A_{eff}/A is as small as 0.6 for a lead target. We call this effect 'nuclear shadowing'; it is as if some nuclear matter were casting a shadow on the remainder. In this talk, we shall try to give a qualitative discussion of nuclear shadowing first to see if the observed shadowing effect is understandable. Then we shall give an outline of QCD calculation, and compare the calculated results with the experimental data. Finally, we shall give conclusion[2].

We may look at deeply inelastic scattering off a nucleus in a frame where the nucleus is moving very fast. The nucleons inside the nucleus are moving side by side. If P is the nuclear momentum and p the momentum per nucleon, p is approximately given by $p\approx P/A$ where A is atomic weight of the nucleus. If m is the rest mass of nucleon, we shall take $p=(p+m^2/2p, \underline{0}, p)$ and the virtual photon energy $q=(q_0, \underline{q}, 0)$ with p large and $Q^2=-q^2\approx\underline{q}^2$. In this standard infinite momentum frame, the virtual photon simply measures effective structure function of the nucleus. $F_2^A(x,Q^2)=\sum e_i^2 x q_i^A(x,Q^2)$ where e_i is the electric charge of the i-quark or antiquark, x is Bjorken variable defined as $x=Q^2/(2p\cdot q)$ and $q_i^A(x,Q^2)$ is the number density of i-quark in the nucleus. If there is no shadowing,

$$q_i^A(x,Q^2) = Z\ q_i^{proton}(x,Q^2) + (A-Z)\ q_i^{neutron}(x,Q^2)$$
$$\approx A\ q_i\ (x,Q^2) \qquad at\ small\ x \qquad (1)$$

From *Eq.(1)*, the cross section for virtual photons off a nucleus should vary as A times the photon nucleon cross section. Therefore, nuclear shadowing implies that the nucleons inside the nucleus are not completely independent. This could be understood as follows. In the nuclear infinite momentum frame the nucleus occupies a longitudinal size $\Delta z_A=2Rm/p$ where R is radius of the nucleus. The longitudinal size of a sea quark or gluon of momentum k_z is $\Delta z\approx 1/k_z$. When $1/k_z>2Rm/p$ or $x<1/(2mR)$, all sea quarks and gluons having such x values, and having the same impact parameter, overlap spatially. Such sea quarks and gluons are really a property of the nucleus rather than a property of the individual nucleons. If the number densities of sea quarks and gluons are large enough to have such overlapping, those sea quarks and gluons from different nucleons may interact with each other to reduce the number densities through recombination.

The parton number densities in a hadron can normally be evaluated at energy Q^2 by using the Altarelli–Parisi equation and some given initial distributions at Q_0^2[3]. At very small values of x, the number densities so obtained may become very large[4,5]. Especially, the gluon density has an exponential growth feature when x goes to zero. Therefore, as long as the values of x are small enough, the gluons in the nucleus will be very crowded.However, in order to distinguish gluon recombination in the individual nucleon and nuclear shdowing, we set up a criterion that nuclear shadowing is due to recombination of gluons from different nucleons. That is, the shadowing occurs only when size of those gluons is bigger than size of nucleon(here size of nucleon means size of valence quark). Since the longitudinal size of nucleon is about $\Delta z_n=2rm/p$ where r is typical radius of nucleon, we then expect to see shadowing effect when $1/k_z>2rm/p$ or $x<1/(2mr)$. If taking $r=1\ fm$ and $m=1\ GeV$, we conclude that the shadowing can happen only when $x<0.1$. When x decreases, there is more gluon overlapping, and the shadowing gets stronger. When $x=x_A=1/(2mR)$, gluons from different nucleons are completely overlapped at any fixed impact parameter, and the shadowing finally reaches maximum. This conclusion is qualitatively consistent with experimental data.

In what follows, we shall give an outline of how perturbative QCD works in calculating effect of nuclear shadowing. First of all, we shall give definition of approximation that we used to ensure validness of perturbative QCD in present case. We shall then give a detailed model of nuclear shadowing and corresponding processes that we calculated. We shall give a set of evolution equations of effective nuclear parton distributions as a result of QCD calculation. Finally, we shall numerically solve those equations to get effective nuclear structure function, and compare calculated results with experimental data.

In the nuclear infinite momentum frame, the measurement of nuclear parton number densities at a specific Q^2 and at a small x is a measurement of nuclear partons of transverse size $|\Delta b| \sim 1/Q$ and longitudinal size $\Delta z \sim 1/xp$. If $g^A(x,Q^2)$ is the nuclear gluon number density, $xg^A(x,Q^2)$, calculated by using the Altarelli–Parisi equation, increases exponentially when x goes to an extremely small value. When $(1/Q^2)xg^A(x,Q^2) > R^2$, the parton model is clearly no longer valid. In order to use perturbative QCD and parton picture, we shall restrict ourselves to work in a low density limit, $\alpha(Q^2)xg^A(x,Q^2) << Q^2R^2$. That is, we shall work in a region where Q^2 is large enough and x is not too small, so that the shadowing is not very strong. We shall work in a leading logarithmic approximation for normal parton evolution and in the first nontrivial order for nuclear shadowing.

In this low density limit, we assume a model of nuclear shadowing where the nucleus is viewed as a collection of uncorrelated nucleons with weakly correlated sea distributions. The shadowing comes from recombination of gluons from two different nucleons. In this model, we do not include annihilation of sea quarks and antiquarks because the number densities of sea quarks and antiquarks are much smaller than the number density of gluons in the region that we are interested in.

To get evolution equations of effective nuclear parton densities, we shall calculate the transition of two gluon ladders into a bare gluon cut vertex, and the transition of two gluon ladders into a bare quark cut vertex. More precisely, we look for the lowest order terms corresponding

to those transitions. In a rather complicated Feynman diagram one also faces the possibility of considering many different cuts through the diagram. Fortunately, there are simple relations between the different cuts, the *AGK* cutting rules[4,6]. As far as relative weightings are concerned the cuts through zero, one and two ladders have values $+1$, -4 and $+2$ respectively. We shall only calculate those transitions whose cut is through both ladders.

In the low density limit, we approximate effective nuclear parton densities to include two parts. One part is proportional to A where the nucleons are independent. The other is a correction where the small x gluons from two different nucleons interact. Then we can write the effective nuclear quark number densities $q^A(x,Q^2)$ and gluon $g^A(x,Q^2)$ as

$$xq_i^A(x,Q^2) = A\ xq_i(x,Q^2) - \delta xq_i^A(x,Q^2) \qquad (2.a)$$

$$xg^A(x,Q^2) = A\ xg(x,Q^2) - \delta xg^A(x,Q^2) \qquad (2.b)$$

where $i=u,d,\cdots$ quark flavours. In *Eq.(2)* we used parton momentum distributions instead of the number densities in order to avoid the singularities of the number densities when x goes to zero. By computing those transitions, we get evolution equations of the correction terms[2]

$$Q^2\frac{d}{dQ^2}\frac{1}{A}\delta xq_i^A(x,Q^2) = \frac{\alpha}{2\pi}\int_x^1\frac{dx'}{x'}\{\frac{x}{x'}P_{qq}(\frac{x}{x'})\frac{1}{A}\delta xq_i^A(x',Q^2) + \frac{x}{x'}P_{qg}(\frac{x}{x'})\frac{1}{A}\delta xg^A(x',Q^2)\}$$
$$+(\frac{3}{2}R\bar{n}-\frac{K}{\pi r^2})\frac{1}{Q^2}\{\frac{2\pi\alpha^2}{N(N^2-1)}(\frac{4}{15}N^2-\frac{3}{5})(xg(x,Q^2))^2 + \frac{\alpha}{2\pi}\int_x^1\frac{dx'}{x'}\frac{x}{x'}\bar{P}_{qg}(\frac{x}{x'})x'G_{HT}(x',Q^2)\} \qquad (3.a)$$

$$Q^2\frac{d}{dQ^2}\frac{1}{A}\delta xg^A(x,Q^2) = \frac{\alpha}{2\pi}\int_x^1\frac{dx'}{x'}\{\frac{x}{x'}P_{gq}(\frac{x}{x'})\sum_{i=1}^{2f}\frac{1}{A}\delta xq_i^A(x',Q^2) + \frac{x}{x'}P_{gg}(\frac{x}{x'})\frac{1}{A}\delta xg^A(x',Q^2)\} \qquad (3.b)$$
$$+(\frac{3}{2}R\bar{n}-\frac{K}{\pi r^2})\frac{1}{Q^2}\{\frac{4\pi^3}{(N^2-1)}(\frac{\alpha C_A}{\pi})^2\int_x^1\frac{dx'}{x'}(x'g(x',Q^2))^2\}$$

where $P_{ij}(z)$, $(i,j=q,g)$ are the first order probabilities of normal evolution equations[3], $\bar{P}_{qg}(z)$ and $xG_{HT}(x,Q^2)$ are given by

$$\bar{P}_{qg}(z) = 2\,(\,-2z + 15z^2 - 30z^3 + 18z^4\,) \tag{4}$$

$$Q^2\frac{d}{dQ^2}xG_{HT}(x,Q^2) = \frac{4\pi^3}{(N^2-1)}\left(\frac{\alpha C_A}{\pi}\right)^2\int_x^1\frac{dx'}{x'}\,(x'g(x',Q^2))^2 \tag{5}$$

where $\bar{n}$ and R are the number density of nucleons in the nucleus and the radius of the nucleus respectively. In *Eq.(3)*, we have taken two gluon distribution $G^{(2)}(x,Q^2)$ in the nucleons to be

$$x^2G^{(2)}(x,Q^2) = \frac{K}{\pi r^2}\,(\,xg(x,Q^2)\,)^2 \tag{6}$$

an approximation which is valid in our low density limit. r is the typical hadronic radius $(r\approx 1\ fm)$ and K is a constant factor. We shall choose $K=9/8$ to ensure the correction terms to be equal to zero when A goes to 1.

To solve the evolution equations, we need knowledge of nuclear parton distributions at energy $Q^2=Q_0^2$. These input parton distributions include all nonperturbative information about the nucleus at the scale Q_0^2. In principle, they are not calculable within perturbation theory. Since we have well-measured hadron parton distributions, we feel that reasonable nuclear input parton distributions can be obtained. We define the nuclear input parton distributions as:

$$\frac{1}{A}xq_s^A(x,Q_0^2) = xq_s(x,Q_0^2)\,R_s(x,Q_0^2,A)$$

$$\frac{1}{A}xg^A(x,Q_0^2) = xg(x,Q_0^2)\,R_g(x,Q_0^2,A) \tag{7}$$

where $xq(x,Q^2)$ and $xg(x,Q^2)$ are hadron parton distributions chosen to be the well-known hadron distributions of Duke and Owens(DO)[7]. Those factors $R_{s(g)}(x,Q_0^2,A)$ in *Eq.(7)* are supposed to include all the information of nuclear shadowing at Q_0^2. If we choose $Q_0^2=4\ GeV^2$, which is the same as that used by *DO*, nuclear shadowing should not be very strong, and our low density model of nuclear shadowing should be valid. Hence the shadowing factors can be approximated as

$$R_{s(g)}(x,Q_0^2,A)=\begin{cases}1 & x_n < x < 1\\ 1-K_{s(g)}(A^{1/3}-1)\dfrac{1/x - 1/x_n}{1/x_A - 1/x_n} & x_A < x < x_n\\ 1-K_{s(g)}(A^{1/3}-1) & 0 < x < x_A\end{cases} \quad (8)$$

where the constant factor $K_s=0.10$, determined by fitting experimental data of Goodman, et al. at the scale Q_0^2. Unfortunately, one does not have direct experimental data to determine value of K_g. However, one thing we do know is that the value of K_g should not be bigger than K_s. This is because the shadowing effect in sea quark distributions should be larger than that in gluon distribution. Having nuclear input parton distributions and *D0* hadron distributions, we can numerically solve those evolution equations, and then fit those data by using orthogonal Chebyshev polynomials to get effective nuclear parton distributions.

In *Fig.1*, we plot ratios of structure functions as functions of Q^2 for different nuclear targets at $x=0.02$. The dashed lines correspond to large values of $K_g(K_g=0.10)$, and solid lines correspond to small values of $K_g(K_g=0.02)$. The important conclusion is that the shadowing goes away very slowly when Q^2 increases. That is, we can always find nuclear shadowing effect at large values of Q^2 as long as the value of x is small enough.

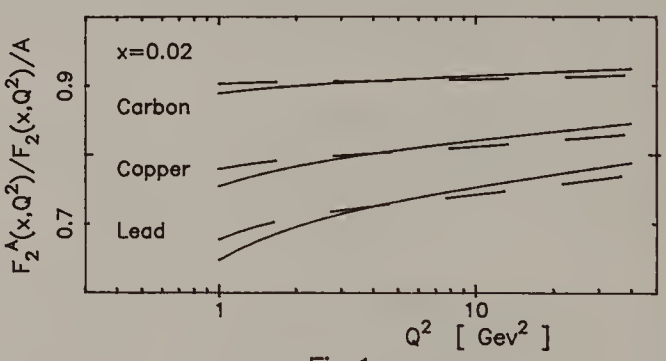

Fig.1

Now we try to compare our results with early experimental data. In *Fig.2*, we plot both experimental data and results(solid lines) calculated by using our effective nuclear structure function. The discontinuities of those lines at $Q^2=3\ GeV^2$ are due to the different range of ν average, where the ν is energy carried by the virtual photon. The ranges of ν average are chosen to be the same as that used in experiment. From *Fig.2*, we can see that nuclear shadowing goes away very quickly when Q^2 increases. However this does not mean that nuclear shadowing will

be a small effect if Q^2 is larger than $30\ GeV^2$ (as seen in the figure)! Actually, the decrease of the shadowing effect is because of the average of ν. The Bjorken variable x is defined as $x=Q^2/(2m\nu)$ with m the nucleon mass. If $Q^2=20\ GeV^2$, the value of x is between 0.05 and 0.25 when ν is averaged between $40\ GeV$ to $200\ GeV$. Clearly, the shadowing effect should be small for such big values of x. The results of our calculation indeed agree with the data.

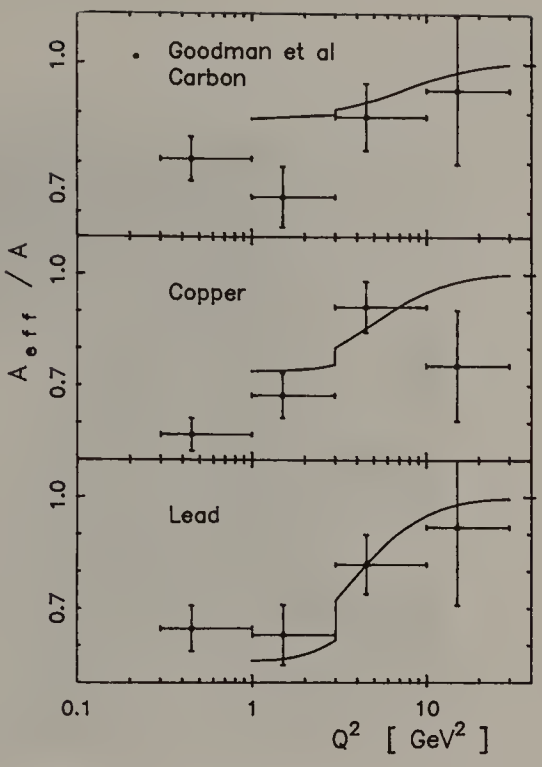

Fig.2

Our overall conclusion is that recombination of gluons from different nucleons is responsible for the observed nuclear shadowing. At small values of x, nuclear shadowing is expected even at large values of Q^2. The nuclear shadowing goes away very slowly when Q^2 increases. Our results of QCD calculation are consistent with early experimental data.

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K. Y. Chen, MC of the CUSPEA Session

Reception at the Chinese Consulate: T. D. and Jeannette Lee with CUSPEA students, participants from the Symposium, the Consul General and members of his staff.

HOT ELECTRON PLASMA EQUILIBRIUM IN THE CONSTANCE B MIRROR EXPERIMENT

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Abstract

Experimental investigation of plasma equilibrium and stability is performed in a quadrupole magnetic mirror. The plasma pressure profile is found to be hollow with a baseball seam structure, which reflects the drift motion of the deeply trapped particles. This hollow plasma is very stable with a decay time of approximately 0.5 seconds, which is close to the collision time scale of the hot electrons (T_e=450keV).

1. Introduction

Since the first minimum B mirror experiment showed dramatic improvement in plasma stability and energy confinement[1], the minimum B configuration has been of particular importance in magnetic mirror research. The configuration has been shown to be stable to all MHD perturbations if the plasma pressure profile is peaked at the center of the magnetic well[2]. This desirable property has been used in the tandem mirror designs to provide overall machine stability. Understanding of the plasma equilibrium and stability in minimum B fields is therefore one of the key issues concerning the physics and the successful operation of tandem mirrors. This paper describes an experimental investigation of plasma equilibrium and MHD stability in the Constance B mirror experiment.

The Constance B mirror is a simple quadrupole magnetic mirror with a baseball magnet (Fig.1). Plasmas are created by up to 4 kW of electron cyclotron resonance heating at 10.5 GHz. The experiment is typically operated at B_o=3 kG and gas pressure $2\times e^{-7}$ to $5\times e^{-6}$ torr,

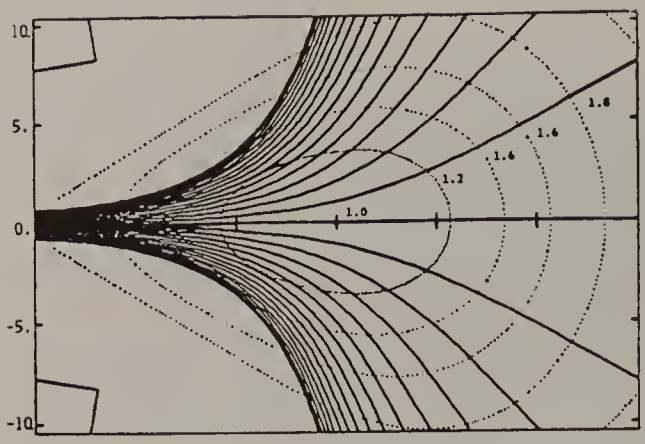


Fig.1 Constance B mirror magnetic field geometry. The dotted lines are constant magnetic field contours.

with a shot length of 2 seconds. Typical plasma parameters are T_{eh} =450keV, $n_{eh}=2\times e^{11}cm^{-3}$, $n_{ec}=2\times e^{11}cm^{-3}$, $T_i\simeq 30$ eV, beta=30%. The plasma equilibrium is studied by a set of experimental measurements. The diagnostics used are: diamagnetic loops, magnetic probes, TV camera and x-ray pinhole camera, skimmer probes, and end loss detectors. A stable, hollow baseball seam plasma equilibrium has been observed in a wide range of parameter regimes in this device.

Because a detailed equilibrium and stability analysis is complicated by the magnetic geometry, we will only concentrate on the experimental observations here. A short discussion follows the next section to draw a complete physical picture from the separate experiments.

2. Equilibrium Observations

Plasma equilibrium is studied by a set of experiments including diamagnetic loops, plasma radiation, skimmer probes, and end loss measurements.

The general structure of the plasma profile can be seen by the visible light and x-ray pinhole cameras looking into the plasma. Fig.2(a) and (b) show visible light photographs taken by a CCD TV camera 30 ms after ECRH turned off, when most cold electrons have been scattered into the loss cone ($\tau_{scat}=50\mu s$), and only the hot electrons are still confined ($\tau_E=0.5$-1.sec). Fig.2(c) shows an x-ray picture taken by an x-ray pinhole camera in the same condition. Both the x-ray and visible lights side view pictures are "C" shaped and the opening of the "C" corresponds to the magnetic fanning where the cameras have the longest line of sight. It suggests that the hot electron profile

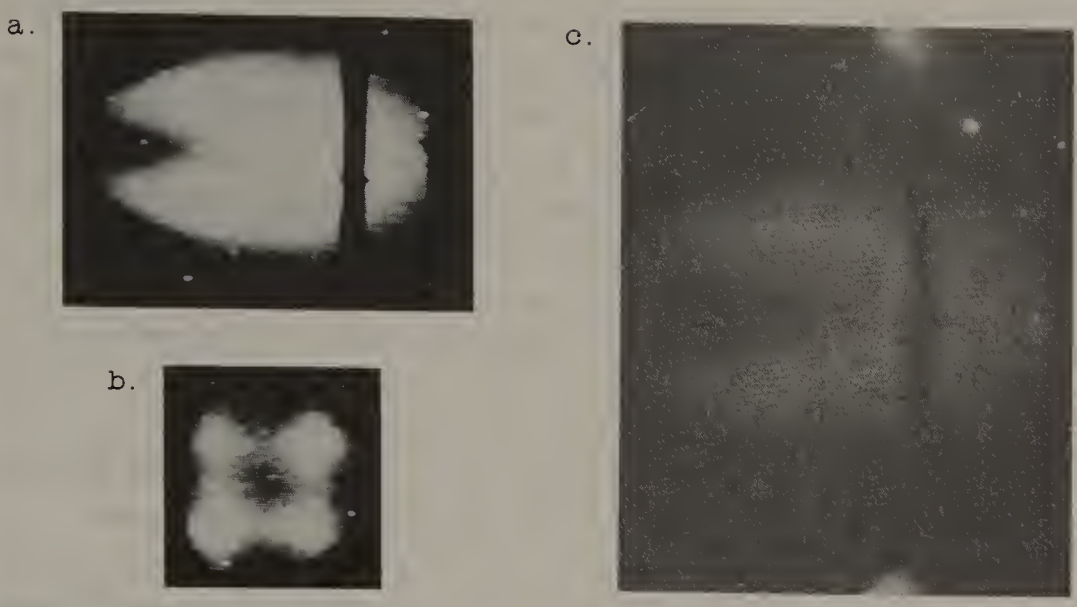


Fig.2 visible lights and x-ray photographs. (a) visible lights side view. (b) visible lights end view. (c) x-ray pinhole side view. The vertical bar is the shadow of the diamagnetic loop.

is hollow and shaped like a baseball seam. It also shows that the plasma is mostly contained inside the resonant surface.

Another strong evidence that the plasma is concentrated along a baseball seam is from skimmer probe measurements. It is possible to insert a 1/4" diameter aluminum probe into the opening of the "C". Fig.3 shows the diamagnetic loop signal vs. the probe's radial position. The radius of the resonant surface at this axial location (z=4") is about 4". The data shows that diamagnetism does not change until the probe reaches the magnetic axis. But no plasma could be generated if the probe was 1/2" off of the symmetry plane.

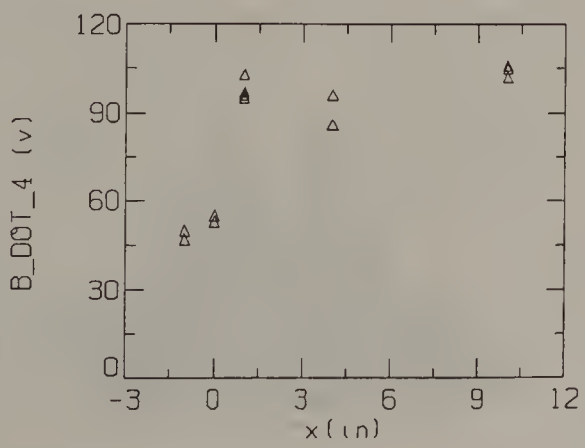


Fig.3 Change of diamagnetism as the skimmer probe moves radially into the opening of the "C" at 4" from the mid-plane.

The equilibrium magnetic field is measured by diamagnetic loops and b-dot probes. The ratio between the diamagnetic loop signal and the b-dot probe signal is independent of the total stored energy, and is a function of the plasma profile only. Diamagnetic field distributions from different equilibrium pressure profiles were numerically calculated and compared with the measurements. The initial calculation showed that the diamagnetic measurement is in good agreement with the profiles which drop about 50% at the magnetic axis.

The hollow plasma profile is also observed by the end loss diagnostics. Fig.4 shows an end loss energy profile measured by a scintillator probe outside the mirror throat. The two peaks in the end loss profile correspond to the plasma peaks at the symmetry plane.

This plasma equilibrium has been observed in a wide range of plasma parameters and for different gases. It is very stable and decays quiescently with a time scale of 0.5-1 second, independent of the ambient gas pressure. Measurements of the diamagnetic fluctuations showed only low level instabilities ($\delta B/B<10^{-3}$) associated with the ECRH heating.

3. Experiment Analysis

The plasma equilibrium is maintained by balancing the plasma pressure and the Lorenz force resulting from the interaction of the plasma current and the magnetic field, $\vec{J} \times \vec{B} = \nabla \cdot \vec{P}$. The plasma pressure is a free function in this equation. It is determined by experimental conditions such as plasma heating, fueling, as well as stability criteria.

The confinement properties of this hot electron plasma are best understood in terms of single particle dynamics. Fig.5a shows the guiding center motion of a deeply trapped, relativistic electron (T=360keV) in the Constance B magnetic field. The electron orbit

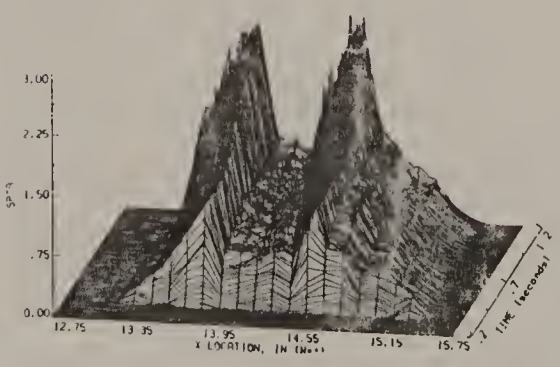
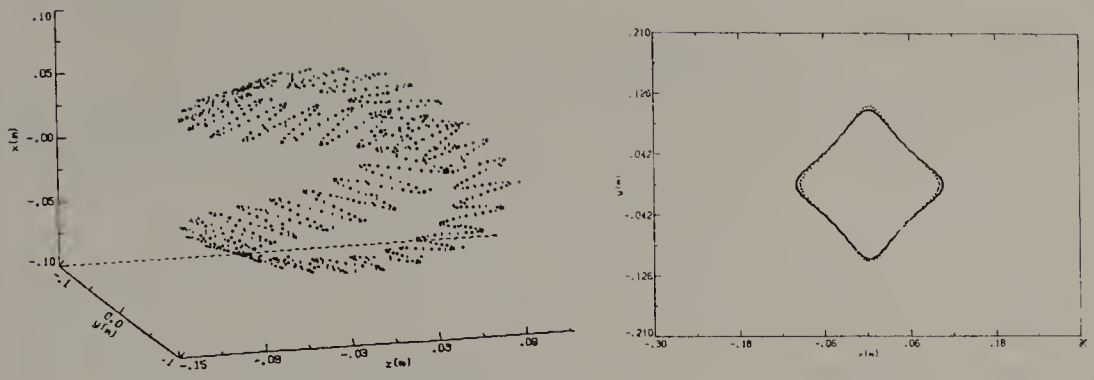


Fig.4 End loss energy profile measured by a scintillator probe.



(a) (b)

Fig.5 Calculated drift orbit of a 360 keV electron in the Constance magnetic field. (a) guiding center orbit. (b) drift surface at midplane.

follows a baseball seam curve, which reflects the geometry of the confining magnet. Fig.5b shows the drift surface of the same electron at midplane. Since this drift surface is nearly closed, the electron will stay on that surface for many passes. It should also be noted that the highest ECRH heating rate occurs where the magnetic field line is tangent to the resonance surface. Mapping of these tangent points on the resonance surface gives a baseball seam curve similar to the electron drift orbit. Particles starting at such field lines will

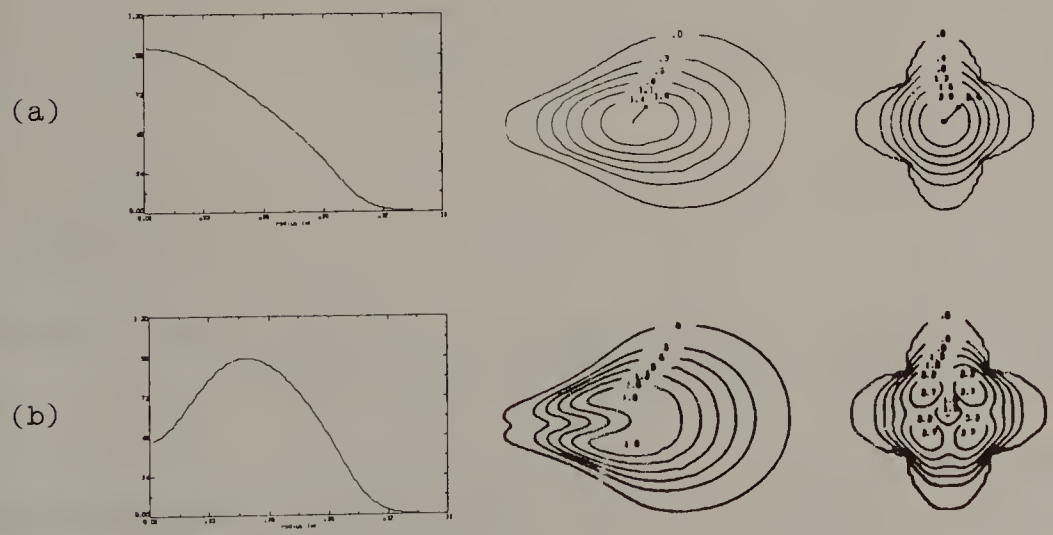


Fig.6 Midplane radial pressure profiles and line integrated side and end views of (a) a peaked and (b) 55% hollow plasma profiles.

be better heated and they will drift on the flux surface along their drift orbits, thus producing the baseball seam equilibrium.

The plasma images observed in this experiment are the projections of the baseball seam structure. The plasma temperature profile has been measured to be flat across magnetic field lines[3]. The x-ray radiation is therefore proportional to n^2. Fig.6 shows the line integrated side and end views of two model plasma profiles. Comparison of the modeling with the measurements suggests a 50% hollowness in the plasma pressure.

Plasma equilibrium is also restricted by stability criteria. A hollow plasma profile in a minimum B magnetic field is susceptible to interchange instability. However, this plasma has been found to be very stable. Possible stabilizing effects were analyzed, and we have excluded the possibility that the stability was due to line tying or FLR effects. The stabilizing compressibility was a significant term but it seemed not able to explain all the observations.

4. Conclusion

A stable, hollow plasma equilibrium was observed in a minimum B magnetic mirror. The plasma was concentrated on a baseball seam curve which reflected the confining magnet. The formation of this equilibrium was due to the ECRH heating profile and the drift motion of hot electrons in the quadrupole magnetic geometry. But the stability of this plasma has yet to be explained.

Acknowledgments

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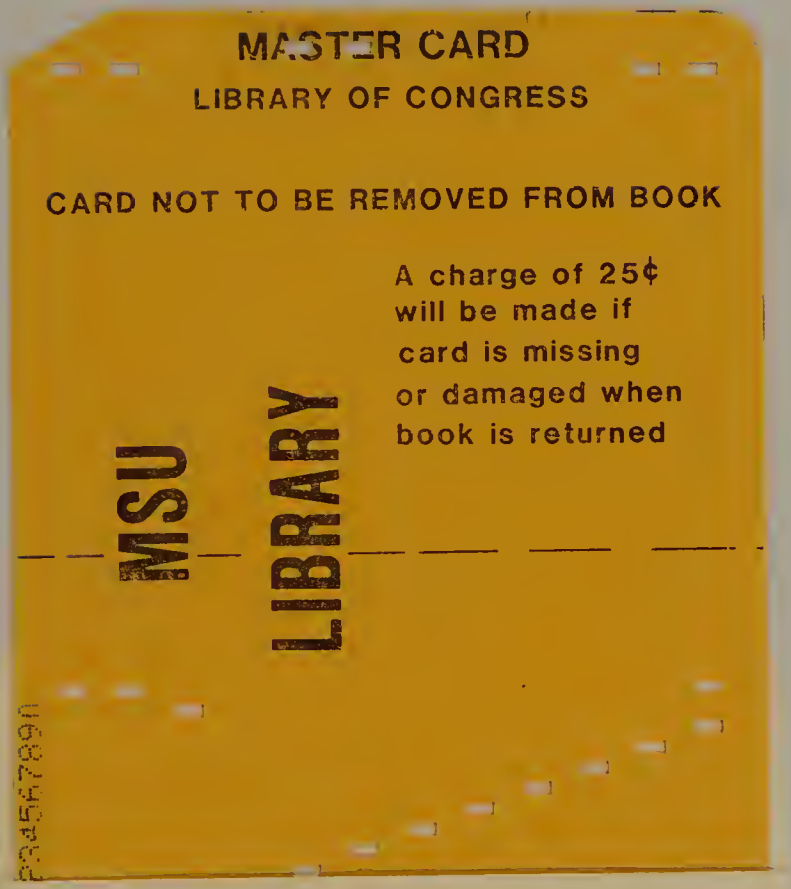

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